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Antiquity

A QUARTERLY REVIEW OF ARCHAEOLOGY

Edited by Glyn Daniel

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Antiquity

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Editorial

PLATE I

ANTIQUITY has had a long tradition of punctual quarterly publication on the first day of March, June, September, and December. It was broken last year by the British printing strike. We regret that this first number of 1961 is also late. It is, moreover the first number to bear the imprint of THE ANTIQUITY TRUST. The lateness and the new imprint are not unconnected.

Since Dr Crawford's death in 1957, ANTIQUITY has been owned by the publisher, Mr Edwards of Ashmore Green, Newbury, Berkshire. On 6 December, 1960, Mr Edwards, who had for some months indicated his readiness to sell the journal, informed the Editor that he did not intend to publish ANTIQUITY after the December number and wished to sever his long association with the journal as from 31 December, 1960. The Editor and Advisory Editors took counsel and decided that this was the moment to see that a journal which over the last thirty years or so has meant much in British archaeology and in the appreciation of British archaeology overseas, should be owned by an independent archaeological body and not by any individual or publisher. To this end subscriptions were invited from archaeologists, archaeological publishers, and journals and trusts interested in archaeology. The response was immediate and heartening and with the money subscribed so generously it has been possible to purchase ANTIQUITY, whose ownership is now vested in THE ANTIQUITY TRUST.

The details of this new arrangement and the names of those who so willingly subscribed to the funds of the Trust will be published in the June number. Meanwhile we wish to record our warm appreciation of the encouragement and help of so many people during a difficult period of transition. The printers have readily co-operated in trying to get the March number out as quickly as possible under difficult circumstances. Will readers please note that W. Heffer & Sons Ltd. are now publishing ANTIQUITY for the Trust, and that all subscriptions, trade enquiries and advertisements should be sent to them at 104, *Hills Road, Cambridge, England.* No communications about ANTIQUITY should any longer be sent to Mr Edwards of Ashmore Green—there are some who, even three years after O. G. S. Crawford's death, still write to him at Nursling about ANTIQUITY.

The change in ownership means no change in the policy or purpose of ANTIQUITY, as we hope this present number will show. It contains many articles of great interest and variety,

and we hope it is not invidious to draw attention to two of these, those of Mr Sinclair Hood, and of Dr Margaret Murray. Mr Sinclair Hood's article is one of those things that Editors dream of; it suddenly appeared through our letter-box on December 1st, the day of the publication of our last issue, with a covering letter saying; 'It does seem time, since Palmer will not or cannot proceed to a formal publication of his theories, that something was published in a reputable journal with a fairly popular appeal like ANTIQUITY to make clear once and for all how unfounded the implications of deliberate forgery or dishonesty against Evans are. I know that Palmer did subsequently make a public disclaimer, but people will go on thinking "no smoke without fire". I would also like to publish precise facts about the context of the fragments of the tablets that we have been finding at Knossos since 1957, since this is relevant to the matter.'

Here then (pp. 4–7) is Sinclair Hood's clear statement of the Linear B tablet problem which readers of *Antiquity* as well as readers of the *Observer* and the *Listener* will find of absorbing interest. We have, of course, offered Professor Palmer room for reply in the June number.

Dr Margaret Murray's article (pp. 8–13) was commissioned after Professor Stuart Piggott had heard a brilliant broadcast by her. She readily agreed to write a backward-looking article on archaeology as she has seen it during a long life. When the article arrived the Editor's curiosity was aroused by the firm dating of the creation (p. 8) and by the age of the author whose work so many people have admired for so long. We asked Dr Murray about the date of the creation and she replied 'I take my evidence from the first chapter of *Genesis* and for exactness it beats C14 into a cocked hat!! *Six p.m.* The periods of creation are divided into "days" and each "day" is said to consist of evening and morning, showing that the "day" began on what we should now call the evening before, e.g., Passover, Day of Atonement, Eve of St John, Eve of All Hallows, Ramadan, and in Palestine, Easter Eve. *Wednesday.* The earth was created on the fourth day of the week, and Wednesday is the fourth day of the week. *March* 21. As the length of days varies according to the season of the year, it is obvious that any calculation must have been made when day and night were equally balanced, i.e. one of the equinoxes. And as the vernal equinox has always been more important than the autumnal, we get March 21. *Therefore*: the full date, 6 p.m., Wednesday, March 21. Though this is a bit of spoof dating, it is quite as good as many dates which are accepted, especially among modern writers on Egyptology.' Dr Murray's article must be the only article, or one of a very few articles, written for ANTIQUITY by anyone over ninety. To the Editor's queries, Dr Murray replied crisply, 'I was born in July, 1863 and am therefore ninety-seven.' We have immediately commissioned her to write another article for the June 1963 number of ANTIQUITY.

Among the many articles commissioned for the 1961 numbers of ANTIQUITY is one by Miss Judith Wilkins on the work of Worsaae in relation to British Archaeology. Worsaae has been described as the first professional in the history of archaeology, and his influence on the development of British archaeological thought in the nineteenth century was great. In the course of her researches Miss Wilkins has drawn our attention to many engaging drawings of archaeology in Denmark in the middle of the 19th century, and we publish in

this number (PLATE I) two of these drawings which seem to look forward to the work of Signor Lerici in the archaeological prospecting of Etruscan tombs.

Congratulations to the Gentlemen of Spalding who in October 1960 celebrated the 250th anniversary of their Society, to the Society for Roman Studies who this year celebrate their fiftieth anniversary, and thirdly to *The Illustrated London News* (not only because its Editor, Sir Bruce Ingram, has been given a D.Litt. *honoris causa* by the University of Oxford) but because it has decided to number its notable contributions to archaeology, and because the first article so numbered, in its issue of 17 December, 1960 was No. 2040. The Spalding Gentlemen's Society was founded in 1711 'for the supporting of mutual benevolence, and their improvement in the liberal sciences and in polite learning'; it claims to be the earliest provincial association for the encouragement of archaeology, the second oldest society concerned with antiquities in Britain, and to have one of the oldest Museums in the country. It celebrated its two-hundred and fiftieth anniversary with an evening reception in its enlarged museum, and a dinner where the health of the Society was proposed by representatives of the Society of Antiquaries of London and of the Royal Society. Long may the Gentlemen of Spalding flourish.

The Society for the Promotion of Roman Studies is celebrating its jubilee by the publication of a special volume (see *Book Chronicle* entry, p. 36 below) inscribed to Miss M. V. Taylor (who was Secretary of the Society from 1923 to 1954, and Editor from 1923 to the present day—a most remarkable achievement), and by an exhibition. Both the Jubilee Volume and the Exhibition will be discussed in the next number of ANTIQUITY.

The special interest of *The Illustrated London News* in archaeology dates from the present Editor's taking over direction of the journal in 1900, and in that year appeared item No. 1 in the long series now well over two thousand; it was a short account of the work done by Flinders Petrie at Abydos on behalf of the Egypt Exploration Fund. It is amusing to turn back to this first archaeological contribution and see the short text and the line drawings cheek by jowl with splendid advertisements for Scrubb's Cloudy Fluid Ammonia, Mrs Winslow's Soothing Syrup, Floriline for the Teeth and Breath, The Mexican Hair Renewer, and the Swan Fountain Pen of which Colonel (as he then was) Baden-Powell is writing 'Your pens were of the greatest use to me during the late operations in Rhodesia, both for drawing and writing: they are just the thing one wants in the field'. It is fascinating to turn through the pages of *The Illustrated London News* in these early years and see how more and more archaeology was put in, until in the years between the wars there were never less than thirty items in a half year.

A correspondent, Mr J. H. Farrer, of Haslemere, takes us to task for quoting from the Michelin *Guides* that St Véran was the highest inhabited village in Europe, and the Col de L'Iseran the highest motor-road, pointing out that Juf in Switzerland is usually regarded as the highest village, and that the highest motor road in Europe is the Carretera de la Sierra Nevada. He would indeed seem to be quite right, and many guidebooks wrong. The *Guide Bleu* gives Juf as 6,997 feet and describes it as 'claiming to be the highest permanently occupied hamlet in Europe', although in a companion volume, describes St Véran as 'claiming to be the highest permanently inhabited village in Europe'. The highway southeast of Granada in the Sierra Nevada reaches the Picacho de Veleta at a height of 10,824. We can hardly wait to visit these elevated places.

The Date of the Linear B Tablets from Knossos

by SINCLAIR HOOD

The Director of the British School at Athens here discusses the date of the Linear B Tablets at Knossos, which has recently been made a controversial issue by Professor L. R. Palmer of the University of Oxford, in articles in the Observer *and the* Listener.

IN an article in the *Observer* for 3 July, and again in the *Listener* for 27 October, 1960, Professor L. R. Palmer argues that the Linear B inscribed clay tablets found at Knossos date not from the end of the 15th century B.C. but from a time some two hundred years or so later. These tablets according to the excavator of Knossos, Sir Arthur Evans, belonged to the age of what he called the 'Last Palace', destroyed about 1400 B.C. But after the destruction of the 'Last Palace' the site was not abandoned; it was reoccupied and inhabited for another hundred years or so. Evans thought that this re-occupation was due to 'squatters' who had made dwellings for themselves among the ruins of the old palace. But the character of what Evans found dating from this 'Re-occupation period' (as he called it) fits better in some ways with the idea of a restored palace than with that of an occupation by 'squatters'. Evans himself was not unaware of this, and once at least hinted at the possibility of a palatial reoccupation, although he subsequently veered away from the idea. In the domestic quarter of the palace he noted 'signs of attempts at restoration on a large scale (i.e. after the destruction of the "Last Palace") which make it probable that dynasts of the old stock still maintained a diminished state on the palace site.'[1] It is therefore possible that Professor Palmer is right in believing that there was another palace on the site of the earlier Palaces at Knossos during the 'Re-occupation period' of the 14th and 13th centuries B.C. But of this latest palace, if palace it was, owing to erosion and stone-plundering through the centuries, comparatively little had survived when Evans began to excavate. It was in any case almost certainly poor and mean compared with the other great palaces which had preceded it on the site. This was indeed a time, the 14th and 13th centuries B.C., of general impoverishment and decline throughout the civilized world including Egypt and the Near East.

Assuming, however, a palace at Knossos during the 'Re-occupation period' it is not impossible that some of the many deposits of Linear B tablets that Evans found date from then. For Evans admits that some of these tablets were not in any well-stratified context, and there is no reason on the face of it why such unstratified tablets should not belong to the period after 1400 B.C. Evans himself at one time thought that some of the Linear B tablets must belong to this 'Re-occupation period'. In an account of tablets found in the 'Little Palace' (a great house lying just west of the palace) in the report on his sixth season of work at Knossos (1905) he writes: 'But what is still more interesting is the evidence, now for the first time supplied by some fragmentary clay tablets found in connection with these sealings, that the fully developed linear (i.e. Linear B) script of Minoan Crete continued to be at least partially in use during the later period. It thus appears that the fall of the palace did not bring with it the absolute extinction of letters, and the true dark ages of Crete were not yet.'[2] In *Scripta Minoa I*, published a few years afterwards (1909), Evans stated the

[1] Evans, *Scripta Minoa I* (1909), p. 53.
[2] *BSA*, XI (1904–5), p. 16.

same view.[3] Eventually, it is true, he reverted to his earliest opinion and thought that all of the Linear B tablets from Knossos must belong to the period of the 'Last Palace' destroyed about 1400 B.C.

Professor Palmer, however, claims that these tablets *all* belong, not to the time of the 'Last Palace', but to the end of the 'Re-occupation period' some 200 years later. To establish his claim he is bound to reject all the more or less explicit testimony published by Evans about the finding of various deposits of tablets among the ruins of the 'Last Palace'. This is a formidable task, but Professor Palmer is not the first person to undertake it. The distinguished excavator of Pylos, Professor C. Blegen, had already advanced arguments for supposing that the Linear B tablets found by Evans at Knossos must date from a period later than the 15th century B.C.[4]

The idea therefore that the tablets found at Knossos date from the period after the destruction of the 'Last Palace' about 1400 B.C. is not in itself new. What is new is the claim made by Professor Palmer that he has found evidence in the original excavation notebooks suggesting that the tablets were actually from later contexts than Evans said they were. The note-books were not kept by Evans himself but by his assistant, Duncan Mackenzie. The deduction made by many people who read Professor Palmer's *Observer* article was perhaps inevitable, that if this were really the case, Evans must have faked or deliberately ignored the excavation evidence. It is only fair to say that Professor Palmer himself has emphatically denied that any such implication was intended.

Until now Professor Palmer has only advanced his arguments for a later dating of the tablets in newspaper articles and on the B.B.C. As he writes in the *Observer* for 3 July, 1960: 'Scholars will wish to reserve judgment until after full publication of the new testimony with its important historical information.' Any attempt to answer his arguments in detail must clearly wait until they are published in a definitive way in a formal article. Meanwhile, however, it is possible to remove certain misconceptions about the facts surrounding the matter that the newspaper articles and B.B.C. talks may have created.

As an example of how Evans misinterpreted the evidence about the context in which the tablets were actually found at Knossos Professor Palmer cites 'a hitherto unknown diary kept by Evans's assistant', Duncan Mackenzie, where the discovery of tablets on a floor together with vases of the 'Re-occupation period' is recorded; while Evans in his final report says that the tablets in question came from an earlier floor dating from the time of the 'Last Palace'. The 'hitherto unknown diary' is one of the Excavation Note-books stored along with the other archives of the Knossos excavations in the Ashmolean Museum at Oxford, of which Sir Arthur Evans was for many years Curator. Professor Palmer describes an entry in this note-book for Tuesday, 8 May, 1900, the first year of the excavations at Knossos. The entry concerns a deposit of tablets found just to the west of the north entrance into the palace, and records how they appeared in large quantities all over a floor on which were also lying vases of the 'Re-occupation period'. Professor Palmer notes that in his great work *The Palace of Minos* Evans says that these 'Re-occupation period' vases were resting on a floor at a higher level, and therefore later, than the floor with the tablets which belonged to the period of the 'Last Palace'. The first volume of the *Palace of Minos* was published in 1921, over twenty years after the beginning of the excavations in 1900; and Volume IV, where the deposit of tablets in question is discussed, was not actually published until 1935.[5]

It is conceivable that observations made by an assistant might for whatever reason come

[3] *Loc. cit.*, p. 55f.
[4] 'A Chronological Problem', in *Minoica* (*Sundwall Festschrift*) (Berlin, 1958), p. 61 f.
[5] *PM*, IV (1935), p. 733 f.

to be overlooked or ignored in a report published twenty or thirty years later. That this is what had happened is certainly the impression conveyed to anyone reading Professor Palmer's *Observer* article. *But in fact this was not the situation at all.*

Evans began to excavate at Knossos in the spring of 1900. Within a few months from the end of his excavations that year he had published a preliminary report of his season's work in the Annual of the British School at Athens. This was the first of a classic series of yearly reports on the progress of his excavations at Knossos which Evans wrote. In this first report on the work of the year 1900 Evans describes how the bulk of the deposit of tablets in question was found *under a wall* of the chamber containing 'Re-occupation period' vases.[6]

On the face of it this might appear to contradict what Mackenzie had written in his notebook for 8 May, 1900. But on page 18 of the same report Evans speaks of some vases found *under a wall*, where from the context it is clear that he must mean *at the foot* of a wall. That this is in fact what Evans meant becomes clear later in the report (page 65 onwards), since it is there evident that at the end of this first season's work in 1900 he thought that the destruction of the palace containing the tablets took place after the Amarna period, in the late 13th or 14th century B.C.

In other words the first person to publish the view that the Linear B tablets of Knossos dated from a late period was not Professor Palmer in 1960, nor even Professor Blegen in 1958, but Sir Arthur Evans himself in 1900!

What then made Evans change his views about the date of the tablets? The answer is simple enough. At the end of the first year's excavations Evans had not yet isolated the destruction of about 1400 B.C. from the final destruction or abandonment of the site at the end of the 'Re-occupation period'. But in the following year, 1901, fresh evidence came to light, including fresh evidence about the tablets in question found near the north entrance. Following the collapse of a mud-built wall of the 'Re-occupation period' it was seen that these tablets were actually resting on an earlier floor than the 'Re-occupation period' vases. In the report for the year 1901 describing this[7] Evans tells us that the floor with the tablets was 'only a few centimetres' below the floor with the 'Re-occupation period' vases, and his report for the previous year, 1900, makes it clear that the whole of this area, very close below the cultivated surface of the ground and near a modern threshing floor, had been much disturbed. There was therefore every opportunity for confused observations when the tablets first began to be discovered here.

This is the kind of situation which is apt to arise on any excavation. There is a somewhat apposite example from the excavations being conducted by the British School under my direction at Knossos now. On the north side of the Royal Road west of the palace we have found scraps of Linear B tablets in successive seasons since 1957. These tablet fragments all come from a great fill which until this year appeared to belong to the period (Late Minoan II) of the 'Last Palace'. But in the light of further work this year it is reasonably clear that the fill in question must date from a time *after*, and not before, the destruction of the 'Last Palace' about 1400 B.C. There is indeed strong circumstantial evidence that the tablets (like much of the pottery from the fill) belong to the period of the 'Last Palace'; but the fill itself is later, even if not much later, in date.

But did Mackenzie perhaps disagree with the interpretation of the new evidence given by Evans? This, improbable enough in itself, becomes still more improbable in the light of what Mackenzie was writing at the time. At the end of the Phylakopi report published by the British School at Athens in 1904 Mackenzie says: 'Thus in no deposit which was recognized as belonging to this period of partial habitation at Knossos (i.e. the 'Re-

[6] *BSA*, VI (1899–1900), p. 44.
[7] *BSA*, VII (1900–1), p. 35.

occupation period') was a single inscribed tablet or seal, broken or unbroken, ever found during the whole course of the excavations there.'[8] Nothing could be more explicit.

But perhaps both Evans and Mackenzie were blinded by some prejudice which warped their interpretation of the evidence? It is difficult to see what motive anyone could have had in the conditions of fifty or sixty years ago for wishing the Linear B tablets to date from one period rather than another. It becomes still more difficult to entertain such an idea in view of what happened at Knossos in 1905, the year following the publication of the Phylakopi report. For, as we have already seen, in that year Evans himself concluded that the tablets found in the 'Little Palace' must belong to the 'Re-occupation period' after 1400 B.C.[2]

Everything, indeed, suggests that Evans and Mackenzie had open minds about the dating of the Linear B tablets from Knossos and interpreted the evidence with impartiality in these early days of the excavations. It is of course possible that they made mistakes, and they may even have assigned this or that deposit of tablets to the wrong period. But Professor Palmer is claiming that *all* of the many deposits of Linear B tablets have been wrongly assigned by them to the time of the 'Last Palace', and that *all* of them really date from a period long after 1400 B.C. This is a very large claim.

There are two possible lines of attack upon the traditional dating of the Linear B tablets from Knossos to the period of the 'Last Palace'. Either the claims made by Evans and Mackenzie that they had found Linear B tablets with objects, or in contexts, of the period of the 'Last Palace' must be wrong—(the attack launched by Professor Palmer is so far mainly along this line). Alternatively the whole or a large part of the assemblage of pottery, stone vases, fresco paintings, seals and seal impressions, etc., assigned by Evans and Mackenzie to the period of the 'Last Palace' ending about 1400 B.C., must be later in date. Professor Blegen in his Sundwall Festschrift article had already moved along this second line of attack by hinting at the possibility that the famous 'Throne Room' at Knossos might belong to a later period than the 'Last Palace' as Evans claimed. Professor Palmer also refers to the 'Throne Room' in his *Observer* article. 'What evidence is there,' he asks, 'for its dating to the period ending 1400 B.C.? Virtually none.' It is true that no decorated clay vases were found in the 'Throne Room', but there were great frescoes of griffins adorning the walls, elaborately carved stone vases resting on the floor, and shattered remains of large oil jars (pithoi) beside them. It is difficult to see how a very much later date than 1400 B.C. can be argued for these and for the assemblage of other objects traditionally assigned to the period of the 'Last Palace'. Of particular importance in this connection are the many clay seal impressions which appear to go with the Linear B tablets.

But a discussion of the archaeological evidence must wait until Professor Palmer has published his theory in a more formal manner. Enough has been said to show that Evans and Mackenzie agreed in their published interpretation of the evidence about the date of the tablets, and that the views on the subject expressed by Evans in the *Palace of Minos* simply repeat what he and Mackenzie were saying in print at the time of the excavations.

[8] Excavations at Phylakopi in Melos, by the British School at Athens. Society for the Promotion of Hellenic Studies, Supplementary Paper No. 4 (1904), p. 271.

First Steps in Archaeology

by MARGARET MURRAY

Dr Margaret Murray retired from the Assistant Professorship of Egyptology at University College, London in 1935. *At the invitation of the Editor of* ANTIQUITY *she looks back, in this article, on a long and distinguished career, which has included at least sixty-six years as a practising professional archaeologist. What she says is, as always, provocative, pungent and pertinent.*

TO trace the rise of archaeology is practically to write the biography of one man, Flinders Petrie.

I first went to University College, London, as a student of Egyptology in January 1894. Petrie was then in Egypt, digging at Koptos, having in the previous year electrified the learned world by his discoveries at Tell el Amarna. Dr J. H. Walker was in charge of the Egyptology department (then and for years afterwards known as the Edwards Library), and Mr F. Ll. Griffith of the British Museum came twice a week to take a class in hieroglyphs. When Miss Amelia B. Edwards founded the Edwards Professorship of Egyptology, it was with the intention that it should be for the training of students in Egyptian archaeology as distinct from the Egyptian language. She had a small but well chosen collection of Egyptian antiquities which she bequeathed as a nucleus of a teaching collection, and with it her Egyptological library. One show case held the collection, and two bookcases held the library. There were also three 'cradles' which held the enormous tomes of Rosselini, *La Description de l'Egypte*, and Lepsius's *Denkmäler*. It is difficult for any modern archaeologist to realize how few books there were on any archaeological subject. For Egypt there were the early volumes of the 'Egypt Exploration Fund (including the disastrous *Bubastis*) and Petrie's early volumes, including *Kahun*, with its shattering suggestion that certain peculiar painted pottery found in a Middle Kingdom site (*c.* 2000 B.C.) came from the Aegean. Mariette and Heinrich Brugsch were well represented in the Edwards Library, and the Proceedings of the Society of Biblical Archaeology was the chief journal in which to publish short articles. Erman's *Aegyptische Grammatik*, with its English translation by Breasted had just appeared and was being carefully studied by all Orientalists. Egyptian history began at the 4th dynasty, nothing was known of anything beyond Khufu and the Great Pyramid, except the fragments of Manetho's history, the lists of Kings, of which the Tablet of Abydos was one, Herodotus, Josephus, and a few scattered items in other ancient authors. The Biblical record concerning Egypt dates from Abraham's visit, but as the ruler of that country is called Pharaoh without his personal name the exact date is uncertain, but is definitely after the Old Kingdom.

But to understand fully the conditions in which modern archaeology first took shape it is necessary to know the background. Prehistorians were already making their theories felt, though they were regarded as atheists because they were dating their finds before the Creation, which everyone knew took place in 4004 B.C.[1] It also savoured of that impious

[1] To be really exact it took place at 6 p.m. on Wednesday, March 21, 4004 B.C. The evidence for these dates is entirely documentary. The date of the year as calculated by Archbishop Ussher is obtained by taking the Jewish date of the Flood—2348 B.C.—and adding to it in succession the age of each patriarch when he begat a son. The other items can be worked out from the data given in the first chapter of Genesis.

Darwinism. Petrie, however, had been working, publishing, lecturing and exhibiting his finds for ten years, and there was beginning to grow up a new generation of archaeologists, under his influence, though he encountered strong opposition in many quarters. The religious world was shocked at the 'sacriligious robbing of graves', and was violently opposed to any open or even implied contradiction of the Biblical statements. Even I, in my first year as a student, was implored by relatives of an older generation than myself to 'give up Egyptology', for 'that way lies infidelity'. There was the same kind of outcry, though in milder form, against 'robbing the dead', when the tomb of Tutankhamen was found nearly thirty years later. In some quarters it was then said that the King (George V) should stop this violation of the dead. I always wondered whether these indignant people stopped to consider how anatomy schools are supplied. Another body of opponents was the classicists, who, though fewer in number, were as violent and as vocal as the religionists. The whole attitude towards archaeology has undergone a complete change, due in large measure to the work of Flinders Petrie. Like Darwin he fought for the truth against embattled prejudice. In short, one has only to follow Petrie's career to realize that the development of archaeology as distinct from the old fashioned idea of antiquarianism is due entirely to him. He was the first to call in the aid of science by having his metal finds analysed, the first to use pottery as a method of dating, the first to show that every hieroglyphic sign was originally the picture of some object. In fact he was the pioneer in almost every aspect of Egyptian archaeology, and his discovery of the unit of measurement used by the builders of Stonehenge and other megalithic structures showed to the world a new method of archaeological investigation which did not require the aid of the written word.

I had come into archaeology as a perfect ignoramus as regards that subject, so that I did not realize at the time the importance of Petrie's finds at Koptos and Naqada, until Elliott Smith (then Professor of Anatomy at University College) made in *Nature* a sudden and rather savage attack on Petrie's work at Naqada, especially on the evidence for the dismemberment of the bodies and for cannibalism. Petrie was greatly upset, for the article was a bolt from the blue, but he wrote a placatory letter to Elliott Smith offering to show him all the evidence. The answer (I read the letter) contained little more than one sentence: 'I don't care what evidence you show me, I shall not believe it.' This was, of course, the beginning of a feud, but feuds are not uncommon in small communities, and are not unknown even among scientists. When I became the Secretary to the Board of Studies in Anthropology, I found that there were four pairs who were at daggers drawn, and it became my chief anxiety to get them so arranged round the table that they could not see each other and make scathing remarks to the enemy. I learnt then that though women are called cats it is the tom-cats who are the real fighters with their tongues. Elliott Smith, however, was merely the mouthpiece for a jealous Egyptologist, who was once rebuked at a large archaeological meeting where he was holding forth against Petrie, by the Chairman, D. G. Hogarth, with the words, 'But you must remember, Dr . . . , that you climbed into archaeology on Petrie's shoulders.' The campaign both open and whispering continued against him all his life, and reached its triumphant apex when in his old age the archaeological authorities refused him permission to excavate in Palestine and persuaded the French to do the same in Syria. Yet none of these same authorities made any protest over the disastrous excavations at Byblos. I once asked D. G. Hogarth what he thought was the cause of the campaign against Petrie, and he replied, 'Petrie has a pretty wit and is not afraid to use it.'

The finds at Naqada altered the whole aspect of Egyptology. Petrie's previous work had roused so much interest that anyone with any pretensions to culture had some knowledge of the finds. But all those early finds were of periods after 4004 B.C. and therefore well

after the accepted date of the Creation. And now the Biblicists were confronted with a civilization before that great event.

M. de Morgan, then director of Antiquities in Egypt, was the first to point out that the Naqada finds were 'prehistoric'. Petrie in his *Diospolis Parva* proved by his Sequence-Dating method that there were two distinct periods and two distinct cultures. *Diospolis Parva* is unfortunately a very rare book, for the Egypt Exploration Fund was still governed by the idea that inscriptions were of paramount importance and that pottery and other grave-goods were trivial in comparison. The Society was also engaged in clearing and restoring the Temple of Deir el Bahri, which was really a work that should have been undertaken by the Government and not by a private society. So the edition of a volume of *Deir el Bahri* was a thousand, and the edition of *Diospolis Parva* was two hundred and fifty.[2] Years later when economic pressure was driving people out of houses into the restricted quarters of flats, one could get the volumes of *Deir el Bahri* at a shilling apiece at the secondhand booksellers while *Diospolis Parva* seldom appears in the catalogues, and is then snapped up at once at a price prohibitive for the ordinary student.

When Petrie was working at Qau, Miss Caton-Thompson, who was one of the party, excavated the site of Badari. Her careful excavation proved that Petrie's method of Sequence-Dating was borne out convincingly by the stratigraphy. It was then that Petrie altered the nomenclature of the predynastic periods, naming them after the principal sites where the objects of each period had been found, as in the French method of naming the palaeolithic cultures. Thus, Badarian, Amratean, Gerzean and Semainian. Unfortunately, the Egypto-linguist, whose main training has been in the language, has never grasped the importance of a clear distinction of periods, and can still write of 'the Naḳada period' (the cloven hoof of the linguist can be seen in the dotted ḳ), and of Naḳada I and Naḳada II, ignorant or oblivious of the fact that the two cultures of Naqada (phonetic spelling) are entirely distinct, and that a number after the name of the culture indicates that No. 2 is a later form of No. 1. The complete destruction of the Amratean culture by the Gerzean is not paralleled again throughout Egyptian history until the Arab civilization overwhelmed the ancient Egyptian.

Petrie's next dig was at Abydos, where he followed an excavator who was more of a treasure-hunter than a searcher for knowledge, and who in his own account of his clearing of the royal tombs records that he took away from them all the stone vases that were in good condition and left all the others 'en miettes'; in other words, he smashed them up so that the local dealers should not get anything saleable and so spoil the market. His finds were afterwards sold by auction in Paris and fetched good prices. Such a condition of things could hardly occur at the present day. Petrie, however, thought it might be worth while to go to Abydos, for on so rich a site the gleanings after such an excavation would yield a good result. That he was justified in his conjecture, is seen in his two volumes, *The Royal Tombs of Abydos*. From the objects rejected and treated as worthless by the previous excavator, things like the clay sealings for wine jars, Petrie obtained the names of all the kings of the 1st dynasty, and was able to arrange them in their correct sequence. Two kings of the 2nd dynasty were also placed in their correct historical position. This remarkable piece of historical reconstruction was almost entirely archaeological (linguists, please note), for the inscriptions consisted only of the names and titles of the kings, with perhaps two or three signs on a stone bowl or an ebony label to indicate its use.

This was the last of Petrie's spectacular finds, for as he said to me, 'I have now settled the sequence of civilization in Egypt, which is what I set out to do. There are still a few

[2] I suggest to the Egypt Exploration Society that a re-issue of *Diospolis Parva* would be of the greatest value to all Egyptologists.

gaps to fill which other people can do, but the main lines are fixed.' This was perfectly true, though the actual length of time might be a matter of dispute, the chronological sequence was clear, the only gap of any size was the origin and civilization of the Hyksos, who clearly came into Egypt from Palestine.

The importance of Egypt to archaeology is twofold. First: the aridity of the soil and atmosphere in Upper Egypt has preserved organic materials which in the moister conditions of other countries have perished long ago. Even the bread of the predynastic period has survived. Second: the dividing of that history with its immensely long list of kings into dynasties, each of which is numbered in sequence, makes it easy to follow the succession of events and the varying conditions of culture through almost the whole length of Egyptian history. This was the standard by which the great civilizations discovered later—Crete, Ur, etc.—could be placed in their contemporary positions. Only those who have suffered as I have done from 'chronological cranks' can realize what this means.

To turn to other fields: there was no teaching anywhere of archaeology as we know it now. Biblical archaeology was beginning, but was apt to be equated with the Higher Criticism, which was proving that the Old Testament was a compound of different periods and not a complete work composed under the direct influence of the Almighty. This feeling towards the Bible was a real and honest belief and any 'meddling with the Word of God' was the cause of much unhappiness in many hearts. In the classical studies the Greek art and literature were all that was considered worth notice, and the dazzling history of Rome, with its widespread conquests, brought the study of that period into prominence. But excavation was entirely haphazard, it was little more than treasure-hunting.

It required three hard knocks to rouse the classicists from their deathly slumber. The first was Petrie's discovery of Naukratis, the second was Jane Harrison's *Prologomena*, and the third was Arthur Evans's discoveries in Crete. The Palestinian archaeologists had been quick to realize the importance of Petrie's work, for in 1890 he went at the invitation of the Palestine Exploration Fund to work out certain problems in that country. At Tell el Hesy he was able to establish the sequence of pottery from 2000–500 B.C., which was the standard with a few adjustments and enlargements, for later Palestinian archaeologists. Unfortunately, the opposition to his work in Egypt was due to the anti-British feeling among the French authorities, who, until Maspero became Director of Antiquities, seem to have delighted in a policy of pin-pricks and obstructions. But in spite of some set-backs his methods steadily gained ground; the younger generation of archaeologists judged by the results, that his method could always be certain of adding to the store of knowledge, even in the most disappointing excavation, and that an object considered valueless by the unprogressive excavator, might, when seen in its correct sequence, be of great importance.

It must also be remembered that anthropology was also making itself felt, especially after it was realized that officials and even missionaries, in our overseas dominions and colonies, were more successful in their work if they had had a training which would give them a sympathetic understanding of the peoples with whom they would have to deal.

Petrie's work and his lectures over the country were rousing interest, and before the turn of the century, I was sufficiently advanced to catalogue the Egyptian antiquities in the Museum of the Society of Antiquaries of Scotland in Edinburgh. When I remember how little I knew, I feel shocked to think that I had the temerity to undertake such a job, but at any rate I knew more than anyone else except for the few who were actually working with Petrie. Later on I catalogued the Egyptian collections in the National Museum of Ireland in Dublin, the Royal Scottish Museum in Edinburgh, the University Museum in Manchester, and the Ashmolean Museum in Oxford.

The systematic training in archaeology as a University subject began in the Egyptology

department of University College, London. And here, if I may be allowed to blow my own trumpet, I took part. And later on I had the triumph of being told by my colleagues that of all the departments in the College, the Egyptology department was the most truly educational. The course comprised lectures and classes on history, religion, social conditions, language (hieroglyphic and Coptic), dating of objects, anatomy (skeleton and skull measurements), mineralogy. Under the head of language came the actual meaning of the hieroglyphic signs as shown by the objects represented; under the head of Social Customs came all the forms of Art, and domestic life, besides governments, laws, education, trades, and everything that could not be conveniently classed under other heads. For anatomy, Dr Douglas Derry (afterwards Professor of Anatomy in the Cairo University) undertook the training; for mineralogy, Professor Garwood of the Geology department allowed our students to attend the appropriate lectures, for the dating of objects Petrie gave the training while he was in England, I did it when he was abroad; for the language, Dr Walker was an enthusiastic and very efficient teacher. All the rest of the subjects were on my shoulders. And I can safely say that any budding archaeologist that passed through the course at University College realized that archaeology was a serious subject and a life-long study.

The whole course was designed to give a student some knowledge of the various aspects of archaeology. Like all such courses only the foundations of the subject can be laid, but if these are accurate as far as the facts are known, the student has nothing to unlearn if he wishes to continue his study. I could never agree with Petrie that it was better to start a man on field work who was entirely ignorant of archaeology, though I was and still am in accordance with him if the student has had a preliminary training in the language.

In viewing the present state of archaeology as compared with the earlier conditions, it is true that more facts have been discovered, more books have been written, more openings for earning a living have been found, more professorships founded, more departments and even institutes of archaeology opened; in fact it is all very much better organized. But with organization has come a desire for uniformity and a consequent tendency towards rigidity. We are now ruled by that blessed word 'scientific', and 'scientific archaeology' is the order of the day. In this ardent desire to be regarded as 'scientific' archaeology appears to have lost its humanity. It is losing sight of its true aim, an understanding of the human being in all his aspects, physical, mental and spiritual as manifested in the past. The details of the subject should not overwhelm the wider view, otherwise there will be no archaeologists, but a group of technicians only. This is to my mind one of the chief difficulties in the training in universities. Too much stress is laid on the details with consequent overclassification. In the case of the flint tools, it is not the mere manufacture of the tool itself that is of the first importance, it is only subsidiary to the purpose for which the tool was made. What, for instance, could a man make with a hand-axe?

Another vexed question in the study of the past is the relative importance of archaeology and language. I naturally look upon this from the point of view of an Egyptologist. As long as the student is dealing with periods before the invention of writing the question does not arise. But when a civilization has advanced to the point of producing documents, in the form of inscriptions on stone, writing on papyrus, or other material, the problem becomes acute. To my mind, and I have had a long experience in observing the effects of the teaching of archaeology in other countries besides Egypt, the two subjects—linguistics and archaeology—are so different in their aim and in their method that it is impossible to study both equally. It is necessary that an archaeologist should have enough knowledge of the language to recognize the date of an inscription, the formulae, royal names and titles, and names of persons and places. But it is waste of his time to attempt a worth-while trans-

lation of a long inscription or a written document. It is surely the part of the linguist to produce reliable translations which the archaeologist can trust and can quote from freely. The linguist should have a parallel amount of archaeology, otherwise his interpretation of the contents of the document may be misleading, for his work lies in the written word only and not in the living human being.

The trend of all knowledge at the present is to specialize, but archaeology has in it all the qualities that call for the wide view of the human race, of its growth from the savage to the civilized, which is seen in all stages of social and religious developments. Archaeology is the study of humanity itself, and unless that attitude towards the subject is kept in mind archaeology will be overwhelmed by impossible theories or a welter of flint chips.

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Roman Dams in Tripolitania

by CLAUDIO VITA-FINZI

The author of this article is engaged in research on post-classical geological changes in Tripolitania. It is hoped to publish shortly a further article on the Roman dams of the Wadi Megenin by Mr Vita-Finzi in collaboration with Mrs Olwen Brogan.

THERE are many Roman dams in the Tripolitanian province of Libya; only a few of them have received the attention they deserve.[1] This article attempts to explain the variety of purposes served by some of the numerous barrages to be found in a small part of northern Tripolitania.

The earliest thorough study of ancient hydraulic works in north Africa was made by La Blanchère[2] in the Enfida of Tunisia. More ambitious surveys have been organized by Gsell[3] in Algeria and Gauckler[4] in Tunisia; they were based on somewhat superficial observations made by local officials and soldiers. Air photography has made the objective recording and presentation of intricate irrigation systems easier, especially when it is in the hands of experienced and patient observers in low-flying aircraft[5]; and this helps us to work out the function of barrages associated with the canals and ditches.

The ancients built dams in other parts of the dry zone lying south and east of the Mediterranean, in conjunction with schemes of irrigation or town water supply. A few of them are very old[6]; while in places the Romans took over a perfected system of soil and water conservation, complete with dams that merely needed repair and maintenance.[7]

Most of the Tripolitanian dams are patently Roman in their solidity and the lime mortar that binds them, and in being associated with farmhouses or other ruins of the period. They vary greatly in size and shape, however. It is at present impossible to date them precisely, but adequate resources and manpower would have become available during the

[1] Dams are mentioned briefly in the works of Barth and other early travellers in Libya; for summary explanatory accounts, see A. Merighi, *La Tripolitania Antica* (Verbania, 1940), II, 71–8 and bibliography; and references below to works by Oates, Goodchild and Haynes.

[2] M.-R. du Coudray la Blanchère, *L'aménagement de l'eau et l'installation rurale dans l'Afrique ancienne*, Nouv. Archiv. des Miss. Scient., t. vii (Paris, 1897). He cites earlier works, including that by L. Carton (1888) in Southern Tunisia.

[3] Stéphane Gsell, *Enquête administrative sur les travaux hydrauliques anciens en Algérie*, *ibid* (1902).

[4] Paul Gauckler, *Enquête sur les installations romanes en Tunise, I: La Byzacène Orientale* (1897); for a recent account, J. Poncet, 'Les anciennes méthodes d'hydraulique agricole . . . en Tunisie', in *Terre de Tunisie* (Tunis, 1958).

[5] For instance Jean Baradez, *Vue Aérienne de l'organisation Romaine dans le Sud Algérien* (1949) and 'Réseau routier . . . d'exploitation de la zone arrière du limes de Numidie', Actes, *Études des Frontières Romaines* (Bâle-Rheinfelden 1957); and also A. Poidebard, *La Trace de Rome dans le désert de Syrie* (Paris, 1934).

[6] G. W. Murray describes one of the oldest in 'Water from the Desert: Some Ancient Egyptian Achievements', *Geog. Jour.*, CXXI (1955), 171–81. See also Robert Brittain, *Rivers and Man* (Longmans, 1958); H. St John Philby, *The Land of Midian* (1957), etc., *passim.*

[7] The achievements of the Nabateans in the Negev are described by Nelson Glueck, *Rivers in the Desert* (1959), especially 92 *et seq.* Strabo may have been referring to an early dam on Wadi Caam when he wrote (*Geography*, **17**, 3, 18) about 'a kind of cross-wall which the Carthaginians built, wishing to bridge over some gorges . . .'

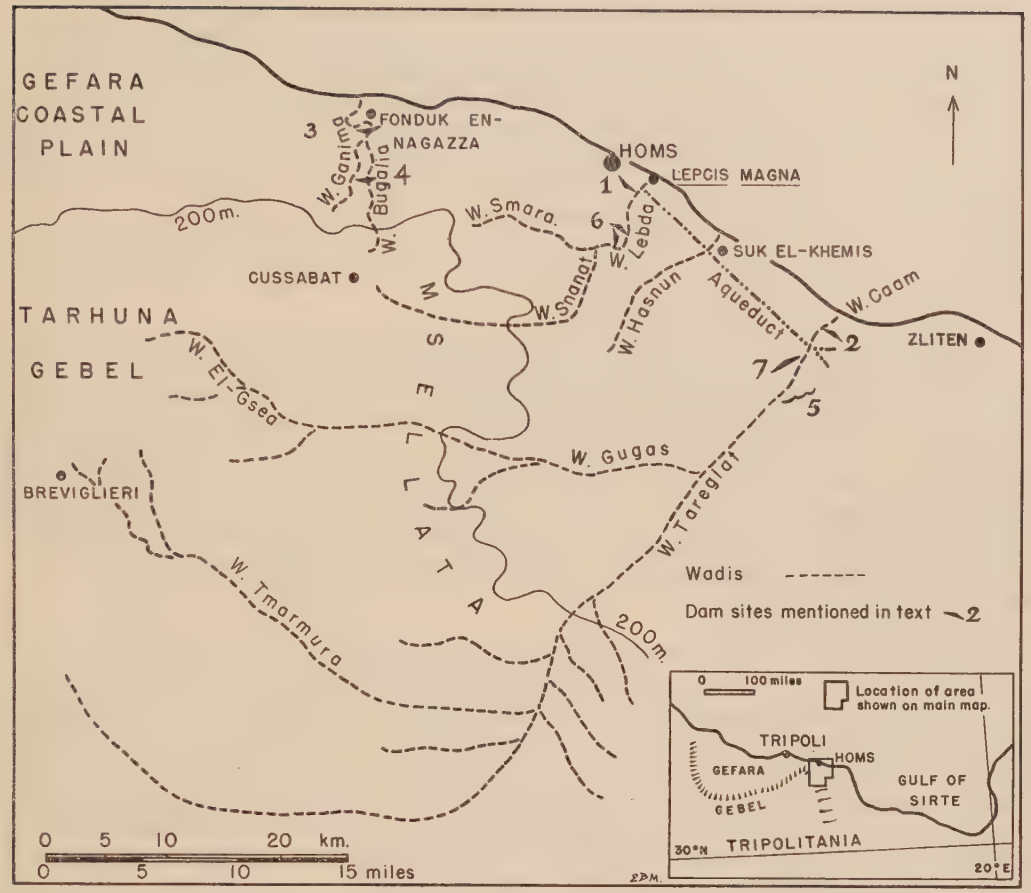

Fig. 1.

2nd and 3rd centuries A.D., when Tripolitania enjoyed its greatest prosperity. Their uniform construction may perhaps be ascribed to the control exercised by Rome over works carried out by provincial corporations, especially during the 2nd century A.D. Large structures were built by organized labour, drawn from two chief sources: the legions and forced-labour gangs.[8] Furthermore hydraulic engineering enjoyed a high repute and an enduring tradition in Roman Africa.[9] Nevertheless, terraces, small earth-dams across gullies, and other minor works, though essential to the complete success of the big dams, were probably not a Roman innovation; and, unlike the masonry and concrete structures, they have remained a part of the local system of land-use.

The wadi systems of Ganima, Lebda and Caam (FIG. 1) have been selected for study

[8] Auguste Choisy, *L'art de bâtir chez les Romains* (Paris, 1873), 204–5; T. R. S. Broughton, *The Romanization of Africa Proconsularis* (London, 1929); etc. Nerva (A.D. 96–8) is reputed to have instigated huge irrigation schemes in North Africa.

[9] The citizens of Saldae (Algeria) asked for Nonus Datus, Expert from the Third Legion, to go out and help them make both ends of a tunnel they were digging meet in the middle (recounted by Clemens Herschel, *Frontinus and the Water Supply of the City of Rome* (Boston, 1899), 179).

because they are exceptionally rich in dams. These watercourses, dry for much of the year, flow during the rare winter floods through the hilly country of the Msellata, the eastern end of the Tripolitanian Gebel where it approaches the sea near the grandiose ruins of Lepcis Magna. This area, Herodotus's Hill of the Graces, was a great olive-growing area in Punic and even more in Roman times[10]; the port of Lepcis was the major outlet of the oil. The low hilltops today are occupied by numerous ruined olive-presses and the remains of farms, usually accompanied by rock-cut or vaulted cisterns into which runoff was led by means of shallow channels and training walls.

Two of the barrages in the area were built in response to the special needs of Lepcis; the others, like most of the Tripolitanian dams, 'played a vital part in developing (its) agriculture on a scale unequalled before or since'.[11]

The Lepcis diversion dam. A short distance south of Lepcis stands a large buttressed dam (FIG. 1, 1) which diverted the muddy flood waters of Wadi Lebda away from the precious harbour at its mouth, and into a canal which led them around the town and into the sea west of it.[12] There is no evidence for the theory that cisterns in the town or near the dam were fed by water impounded by it, especially in view of the muddiness and unreliability of the floods. Furthermore a line thought[13] to represent the trace of a canal that was intended to lead some of the diverted waters towards the northeast for use in irrigation, has been shown to be a low embankment forming part of a ring of fortifications.[14] This was, as far as can be seen, purely a protective dam.

Wadi Caam reservoir dam. Lepcis was supplied with water by an underground aqueduct from Wadi Caam, 19 km. to the east. Its course is marked on the surface of the ground by a line of manholes, and it crosses the floor of Wadi Hasnun about a kilometre above Suq el-Khemis. On the right bank of Wadi Caam, just below the point where it is crossed by the coast road and opposite the mouth of the Lepcis aqueduct, two other aqueduct intakes can be seen,[15] and the presence of some manhole openings in the direction of Zliten suggests that other settlements besides Lepcis were supplied from this source. Near the intakes, the floor of the wadi drops vertically for several metres below its level at the road; springs flow copiously at the foot of this drop.[16]

There can be little doubt that these springs supplied the water for the aqueducts. The amount brought down by the Caam floods changes greatly from one year to the next; and any reservoir or low-gradient channel (like the Lepcis aqueduct) would be seriously troubled by silting if these waters were led into it. The only problem was how to raise the spring waters to the level of the inlets.[17]

[10] R. G. Goodchild, 'Roman Tripolitania: Reconnaissance in the Desert Frontier Zone', *Geog. Jour.*, CXV (1950), 161–78. Romanelli (*see* note 12) gives many of the classical sources.

[11] D. E. L. Haynes, *The Antiquities of Tripolitania* (Tripoli, 1956), 52.

[12] P. Romanelli, *Leptis Magna* (Rome, 1925), 72.

[13] S. Franchi, in *La Tripolitania Settentrionale* (Rome, 1913), *I*, 63.

[14] R. G. Goodchild and J. P. Ward Perkins, 'The Roman and Byzantine Defences of Lepcis Magna', *Papers of the British School at Rome*, XXI (1953), 45.

[15] One of these has been blocked up by what appears to be Roman *opus signinum*.

[16] It is this lower part, fed by the springs and sometimes by flood- or even sea-water, that gave Wadi Caam the undeserved reputation of being a perennial stream. Haynes too (*op. cit.*, 99) suggests that it was made into some kind of reservoir. A thorough investigation of the site by Sra. B. Crova is to be published shortly in *Quaderni della Libia*.

[17] *Cf.* an inscription from the Baths at Lepcis which reads 'Q(uintus) Serui(r)ius Candidus sua inpensa aquam quaesit(a)m et eleuatam in coloniam perduxit' (*The Inscriptions of Roman Tripolitania*, J. M. Reynolds and J. B. Ward Perkins (Rome 1952), No. 357), dated to A.D. 119.

A tall dam (FIG. 1, 2 and PLATE IV(*a*)) stands half a kilometre below the aqueducts and on the right side of the wadi bottom. It is surmounted by a spillway whose lip, as far as can be judged with the aid of an Abney level, is at the same elevation as the intakes. The spring waters impounded by the dam would create a fresh-water lake from which the aqueducts could be fed by mechanical lifting devices (e.g. water wheels), although its depth would be determined by the pressure of the springs and losses from seepage, rather than simply by

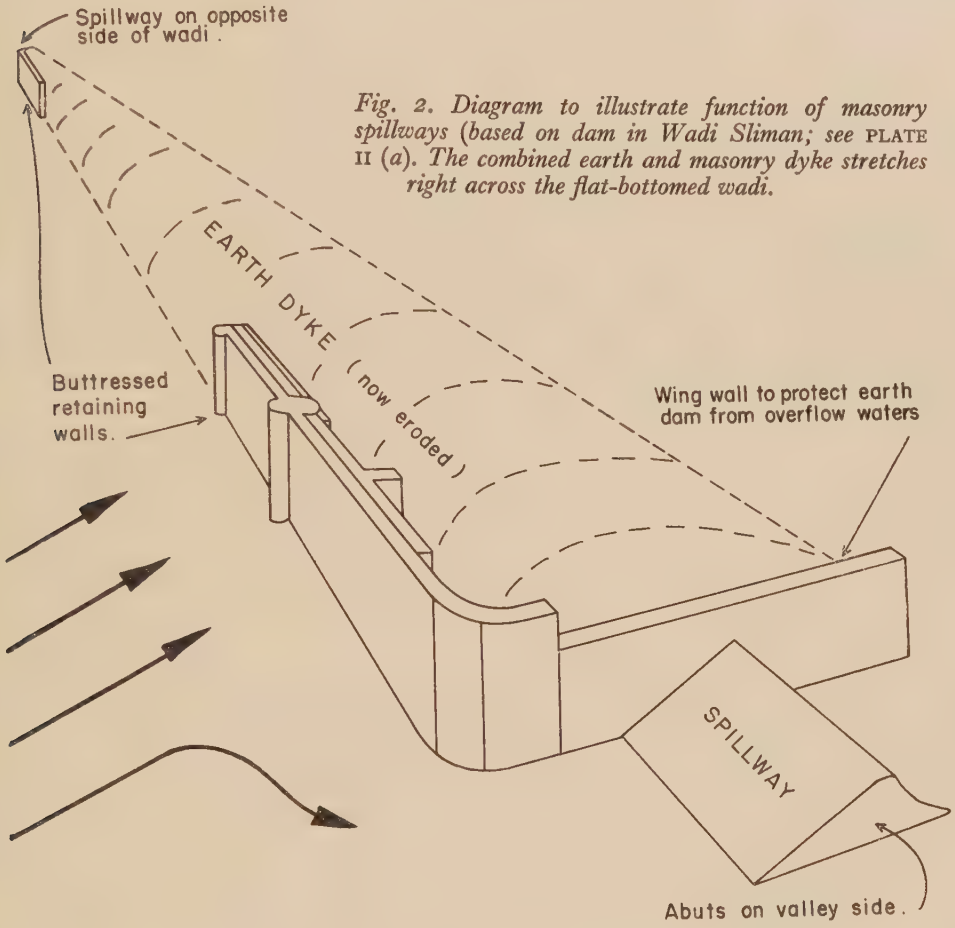

Fig. 2. Diagram to illustrate function of masonry spillways (based on dam in Wadi Sliman; see PLATE II (*a*)*. The combined earth and masonry dyke stretches right across the flat-bottomed wadi.*

the height of the dam. The many other walls and constructions near the road-crossing were probably designed to keep the silt banks from slumping into the water, and to deflect any flood waters away from the reservoir. But other dams further up the Caam and described below reduced the latter hazard to a minimum. The cisterns at Lepcis, which have been mentioned in a previous paragraph, if used as reservoirs fed by the aqueduct, could serve as *castella* or *immisoria*, and cope with peaks in the town's demand for water. It must be pointed out, however, that no connection has yet been found between the aqueduct and these cisterns.

Soil and water retention dams. The same qualities that made the silt-laden spates useless as direct sources of water were put to good use in agricultural development. Although the rainfall of the Msellata was sufficient for olive cultivation on the slopes, other crops requiring moister soils could not be grown by similar 'dry-farming' techniques. As Oates has already shown for Wadi Gsea,[18] dams were built 'to slow down the spate and minimize its capacity for destruction, while retaining just as much water as would sink into the fertile soil thus created'. Once sufficient silt had accumulated, what were formerly irregular and rocky wadi-bottoms could be cultivated with flood-irrigation; this technique is still widely used where the nature of the valley allows it, and the risk of crop-failure because of insufficient or ill-timed floods is no greater than in other kinds of Tripolitanian agriculture.

Where the watercourses were narrow and steep, stone-and-concrete dams were erected and anchored to rocky outcrops; where they were wide and gently-sloping, as in parts of the Middle Tareglat, earth dikes were used. To prevent the earth dam from being washed away by water pouring over it, concrete spillways were incorporated,[19] (PLATE II(*a*) and FIG. 2) often in pairs—one on each side of the valley—with their tops a little lower than that of the earthern sections; any overflow was localized here and directed safely away, perhaps to be used for irrigation. The amount of flow through the sluices could be controlled by wooden gates fitting in slots that can still be clearly seen (PLATE II(*b*)). In fact, all that usually remains of the earth dams are these spillway sections; but sometimes a transverse line of erosion gullies, particularly apparent when seen from the air, marks the edge of the low 'step' of soil that accumulated behind the dike. These gullies show that, now the dams have gone, the original slope of the wadi bottom is being restored.

The stone soil-retention dams are of local limestone; the lower courses of the structure often consist of larger, more carefully coursed blocks than the body of the dam (PLATE III(*a*)). They are generally reinforced with buttresses, and stepped on the downstream side (PLATE III(*b*)), while the upstream side is faced smooth with *opus signinum* to make it waterproof. One dam in the Lebda basin seems to face the wrong way; we can picture the unfortunate engineer who made this error staggering in the August heat, unsure of which way was up- and which downstream, in the twisting, parched bouldery wadi bed he had the accursed job to dam.

Spillways again ensured that overflow was directed where it would not endanger the dam itself or cultivable land downstream; in some places 'catchment walls'[20] diverted the run-off from the hills. Once they had filled with sediment, the dams lost some of their effectiveness in curbing the floods. Furthermore, they no longer formed reservoirs. This was a secondary but important function, as water ponded at the surface would remain for some weeks after the last flood and serve for watering beasts and for supplementary irrigation. Most of the dams show signs of having been raised one or more times, and this usually meant that they had to be thickened or otherwise modified.

Clearly, the higher the dam, the larger the area of cultivable ground created upstream; and, for a certain amount of rise, the degree of gain would be greater for a gently-sloping wadi than for a steep one. This fact made the damming of large watercourses worthwhile, since their beds are generally less steep, as well as being wider than those of their tributaries, and the greater violence and volume of their floods could be reduced in some degree by additional works of water control in the upper tributaries.

[18] David Oates, 'The Tripolitanian Gebel: settlement of the Roman period around Gasr ed-Dauun', *PBSR*, XXI (1953), 81–117. *See* also F. Stroppa, 'L'idrografia della Tripolitania e la politica idraulica romana', *Riv. Col*, XVI (1919), 489–96.

[19] Mr John Stewart, lately of U.S. Operations Mission to Libya, was the first to draw the author's attention to the spillways.

[20] R. G. Goodchild, 'Farming in Roman Libya', *Geog. Mag.*, XXV (1952), 76.

Three of the dams in Wadi Ganima-Bugalia (FIG. 1, 3 and PLATE II(*c*)) ultimately attained over 7 m. in height and accumulated vast amounts of fine silt to produce extensive flat-topped terraces. The other six dams examined in this wadi are lower, but show clear signs of modification during their lifetime.

Besides silting up, other changes had to be contended with. When the wadi was not fixed in position by rocky hills or spurs, it shifted either because it had been deflected by a Roman dam and the alluvium piled up behind it or because of the tendency to migrate that characterizes many desert streams. Thus, when a barrage was anchored at only one end, the other might have to be lengthened to ensure that it remained across the shifting wadi. For instance, one of the dams in Wadi Bugalia (FIG. 1, 4) has been extended down a slope; spillways at its upper end show that the waters once flowed at a higher level. The point is further illustrated by the complicated shape of dam 5 on Wadi Caam PLATE IV(*b*) now several metres above and away from the wadi; and site 6 on Wadi Lebda, where there are two dams on the steeply-sloping northern bank, one built after the other had been rendered useless by a shift in the position of the wadi.[21]

In all there are at least twenty-five stone dams in the Lebda and its tributaries. Enough remains of the dams and the terraces they accumulated to illustrate how effectively a drainage basin might be 'Romanized' into a staircase of cultivable steps. The uppermost headwaters of the Smara and Snanat were dammed and controlled by means of little earth-dams and terraces. Even today the area around Cussabat displays an exceptional concentration of such small-scale works; yet, once the valleys deepen to more than a few feet there is no further attempt at damming, although where the wadi bottom is sufficiently flat and muddy, irrigation can be practised over short stretches without any.

In the Caam basin we find both all-stone and earth-and-spillway dams in large numbers. The upper tributaries of the Tareglat, running in steep-sided valleys, were treated in the same way as the Lebda.[22] Where the valley becomes over a kilometre wide and flat-bottomed, low dikes were sufficient to slow down the waters and induce the accumulation of a greater depth of soil. These gains in rich soil could justify the vast expenditure involved in the erection of Tripolitania's largest dam (FIG. 1), 1½ km. above the aqueduct inlets on Wadi Caam. This massive structure is about 900 metres long and lies almost parallel to the present course of the wadi. It has been suggested that it was built to form a reservoir fed by the Caam's floods and that their silt load filled it in three centuries or so. But it seems more likely that the dam is merely a large soil-retention dam which, besides creating many acres of land, protected the aqueduct works further downstream. The wadi at some time found its way around the dam. There are, furthermore, parts of the southern end which appear unfinished; projecting knobs have been left on which to key the sections still to be added: it may be that shifts in the position of the wadi rendered these portions useless before they had been completed.

It does not necessarily follow that soil-retention dams were as important or numerous in the rest of Tripolitania as in the three drainage basins studied. La Blanchère[23] made the mistake of thinking that what he had seen in one especially favoured area automatically applied to the rest of Tunisia. Nevertheless dams are present in many other Tripolitanian wadis (the Megenin, Uestata and Hira contain some fine examples); while flood-cultivation

[21] C. Vita-Finzi, 'Post-Roman Changes in the Wadi Lebda', in *Field Studies in Libya* (Durham University, 1960).

[22] M. Bradfield and A. Wells recorded eight large dams in the Gsea and twenty in its narrow upper part (Udei el Me). I am indebted to Mrs Olwen Brogan for this information, and for unstinted help at all stages of this study.

[23] P. Gauckler, *op. cit.*, p. 125, points this out.

was and still is a technique widely practised in the broad pre-desert wadis such as those at Ghirza and very notably at Beni Ulid, with low walls serving to spread the waters.

Some authorities[24] suggest that in the pre-desert wadis present-day conditions are less favourable than they were in Roman times. Within the more reliable rainfall zone of the Msellata, however, modern conditions are perfectly adequate for the relative success of this technique. Nevertheless, there are definite indications, in the form of tangible geological deposits, that the behaviour of the streams has changed since Roman times. These successive changes can be summarized as follows.

During the period of dam-building, flow in the wadis was somewhat better distributed through the year than today, partly because of the great attention paid to the control of run-off waters and the large number of trees (largely olive). When the dams and the concomitant works fell into disrepair, the number of trees decreased, and erosion became active both in the wadis and on the hillsides. The wadi floods of today are both more violent and more short-lived than they were two thousand years ago. This conclusion should not be allowed to detract from the achievement of the Romans in making the best possible use of a difficult area, not only by conserving but also by creating cultivable soil.

[24] e.g. R. G. Goodchild, (1950) *op. cit.*, 165.

Book Chronicle

We include here books which have been received for review, or books of importance not received for review, of which we have recently been informed. We welcome information about books, particularly in languages other than English and American, of interest to readers of ANTIQUITY. *The listing of a book in this chronicle does not preclude its review in* ANTIQUITY.

A BRIEF GUIDE TO THE COLLECTION OF IRISH ANTIQUITIES: *By* JOSEPH RAFTERY. *pp.* 96, 10 *plates*, 97 *text-figures. Dublin, The Stationery Office*, 1960, 3*s.* 6*d.* (*Governments Publications Sale Office, G.P.O. Arcade, Dublin*). The first published guide to these extremely rich collections of Irish Antiquities in the National Museum of Ireland. Very good value for money.

INDIAN LIFE IN THE UPPER GREAT LAKES: 11,000 B.C. to A.D. 1800. *By* G. I. QUIMBY. *pp.* 182, 75 *figures. University of Chicago Press*, 1960. $5.95.

LE PREMIER ÂGE DU FER LANGUEDOCIEN. Part III. Les Tumulus, Conclusions: *By* M. LOUIS, ODETTE TAFFANEL *and* JEAN TAFFANEL. *pp.* 423, 204 *figures.* Volume III of the *Collection de Monographes Préhistoriques et Archéologiques* published by the Institut International d'Études Ligures. Bordighera and Montpellier, 1960. 80 N. Francs or 900 lire.

THE EGYPTIANS. *By* CYRIL ALDRED. (Volume 18 in the 'Ancient Peoples and Places' Series.) *pp.* 244, 82 *photographs*, 52 *line illustrations. London, Thames and Hudson*, 1961. 30*s.*

A SHORT HISTORY OF TECHNOLOGY, from Earliest Times to A.D. 1900. *By* T. K. DERRY *and* TREVOR I. WILLIAMS. *pp.* xviii, 782, *frontispiece and* 353 *text-figures*, 8 *chronological tables. Oxford, the Clarendon Press*, 1960. 38*s.*

BABYLONIAN WISDOM LITERATURE. *By* W. G. LAMBERT. *pp.* xix, 359, 75 *plates*, *Oxford, The Clarendon Press*, 1960. £4 10*s.*

[*continued on p.* 28.

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East Anglian Coast-line Levels Since Roman Times

by CHARLES GREEN

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EVIDENCE has for long been accumulating to show that the shores of the Thames estuary now lie at least 15 ft. lower in relation to the sea than in early Romano–British times (summarized, e.g. Wheeler, 1928; Francis, 1932). Francis has suggested that this submergence[1] began early in the 2nd century A.D., its effects being widely felt by the end of the century. Similar evidence has been noted both from the east and south coasts of England though, in the East Anglian 'Great Estuary' (Yare-Bure-Waveney) behind Yarmouth, it had formerly been assessed as about 2 ft. only.

Most writers, however, have been content to assume without confirmatory evidence that these changes took place in a simple progression, so that a graph of them from Roman times to the present day might be represented by a roughly straight line. Godwin, indeed, had sounded a warning note in his analysis of the Fenland deposits at Wiggenhall St German (Godwin and Edmunds, 1933; Godwin, 1940), Jennings had pointed to a 'stand-still' or slight regression after the Romano–British transgression of the Broadland valleys (1952, 50) and a short note by Swinnerton (1955) discussing pottery found below marine clays at Chapel St Leonards, Lincs, came to confirm them. But now substantial evidence from the Yarmouth district has been adduced to show that, after the Iron Age—Romano–British marine transgression, represented by the Broadland 'Upper Clay', land emergence took place during the Saxon period to culminate about the time of the Norman Conquest or soon after, the 'Saxo-Norman Marine Regression' as it has been named (Green and Hutchinson, 1960). Under the South Denes at Yarmouth, a beach with its low water mark now at — 17·5 ft. O.D. has been dated by pottery finds to the 13th century, with a silt covering-layer also containing pottery, inferred to have been deposited by the great flood of A.D. 1287. Supported by a variety of other evidence, they have shown that, at that date, the coast here still stood some 13 ft. higher in relation to the sea than it does today. Thereafter a rapid submergence began which, by Tudor-Stuart times, was much retarded and which today has been reduced to the very low rate of some 1·6 mm. per annum.

The chronology of these changes in the estuary is fairly well established. The beginning of a submergence-phase, marked by the deposition of marine silts (the 'Upper Clay'), began soon after 500 B.C. and reached its maximum probably in the 1st century B.C. During Romano–British times the estuary was still open, though silt banks had accreted, but the land was slowly emerging and mean sea level by the 3rd century was close to that of today. By late pagan Saxon times (*c.* A.D. 600) emergence had progressed and some of

[1] No attempt can here be made to assess the causes of these variations of level, which are probably due to a complex of isostatic, eustatic and other geomorphological changes. The neutral terms 'emergence' and 'submergence', 'marine transgression' and 'marine regression' are in consequence used.

the silt banks were now permanently dry. By the time of the Danish settlement in the area (A.D. 879 or soon after) the emergence was well advanced and the river-channels ran between permanently-dry grass-grown flats. The Bure emptied into the North Sea to the north of the site of the present-day Yarmouth, where now stood an emergent bank of intercalated sands and silts. Cut off from this more northerly stream by the middle grounds, the Yare-Waveney ran out to the south of the Yarmouth bank.

The period of near-equilibrium was now approaching and a sand and shingle spit was growing southward from the Yarmouth bank, to reach a point near Lowestoft by *c.* A.D. 1200. But the scour of the river and the ebb-tide kept this away from the Suffolk shore so that a long narrow channel, 'The Haven', lay between them. To the north of the Yarmouth bank, accretions of sand and shingle were being packed against the estuarine silts on both sides of Bure-mouth, so that this river became more and more impeded. It was gradually deflected southward to cut a new channel through the silts lying inside the growing shingle-bar and finally reached the Yare-Waveney channel and so came to empty into the Haven as it does today. By the time of Domesday, the Yarmouth bank had been settled and no less than seventy burgesses were recorded. In addition, another settlement, that of Little Yarmouth or Southtown, was to grow up during this period across the Haven on the now-dry silt banks filling the southern part of the estuary. So was the pattern of settlement as we have it today finally laid down.

It seems probable that, during the 13th century, a submergence-phase had begun, at first of trifling magnitude, but after the great flood of 1287, it certainly progressed with increased rapidity. It was now that the rivers were embanked to protect the flat pastures and so we have preserved today the approximate drainage lines of the 14th century, though now complicated by the later digging of subsidiary dykes. From time to time, also, artificial channels were cut across the spit south of Yarmouth town to permit entry to the Haven close to the town quays. That still in use was cut in 1560 and, after the provision of protecting piers, the changing current removed the southward-pointing spit beyond, to redeposit the debris at Lowestoft Ness. By the 17th century the submergence-rate had much decreased, a decrease which has continued until today.

It is clear that, though the emergence and submergence rates probably varied slightly at different points along the coast—as indeed they do today (Valentin, 1953)—so considerable an emergence as 13 ft. or more could not have happened in east Norfolk without some corresponding fluctuation both to the north and the south. Direct stratigraphical evidence, such as that observed at the South Denes, can normally be found only by excavation below low water mark. This involves, as there, protection from both tide and seeping water by sheet steel piling or other form of coffer dam and by pumping, resources not normally available for controlled archaeological excavation on the scale required, and so the opportunities offered by the construction of new docks, power stations or the sinking of oil-storage tanks must be taken. It is from such major works, indeed, that most of our available information has been gathered. But secondary evidence, when the need for it is realized, can frequently be found. Furthermore, by inference, the consequences of such emergence may be outlined and many known facts, not before fully understood, may be found to fit those inferences. This paper is an attempt to survey some of this secondary evidence and to indicate where, in future, direct evidence of these post-Roman land-sea oscillations may be observed. Even now it is possible to realize that the interpretation of much of our Saxon, medieval and post-medieval history depends on the physical changes in the estuaries and harbours of our lowland-zone coasts.

One difficulty should first be mentioned. There is a considerable variation in the tide ranges along these coasts. At Yarmouth, the present tide range is no more than 6·7 ft.,

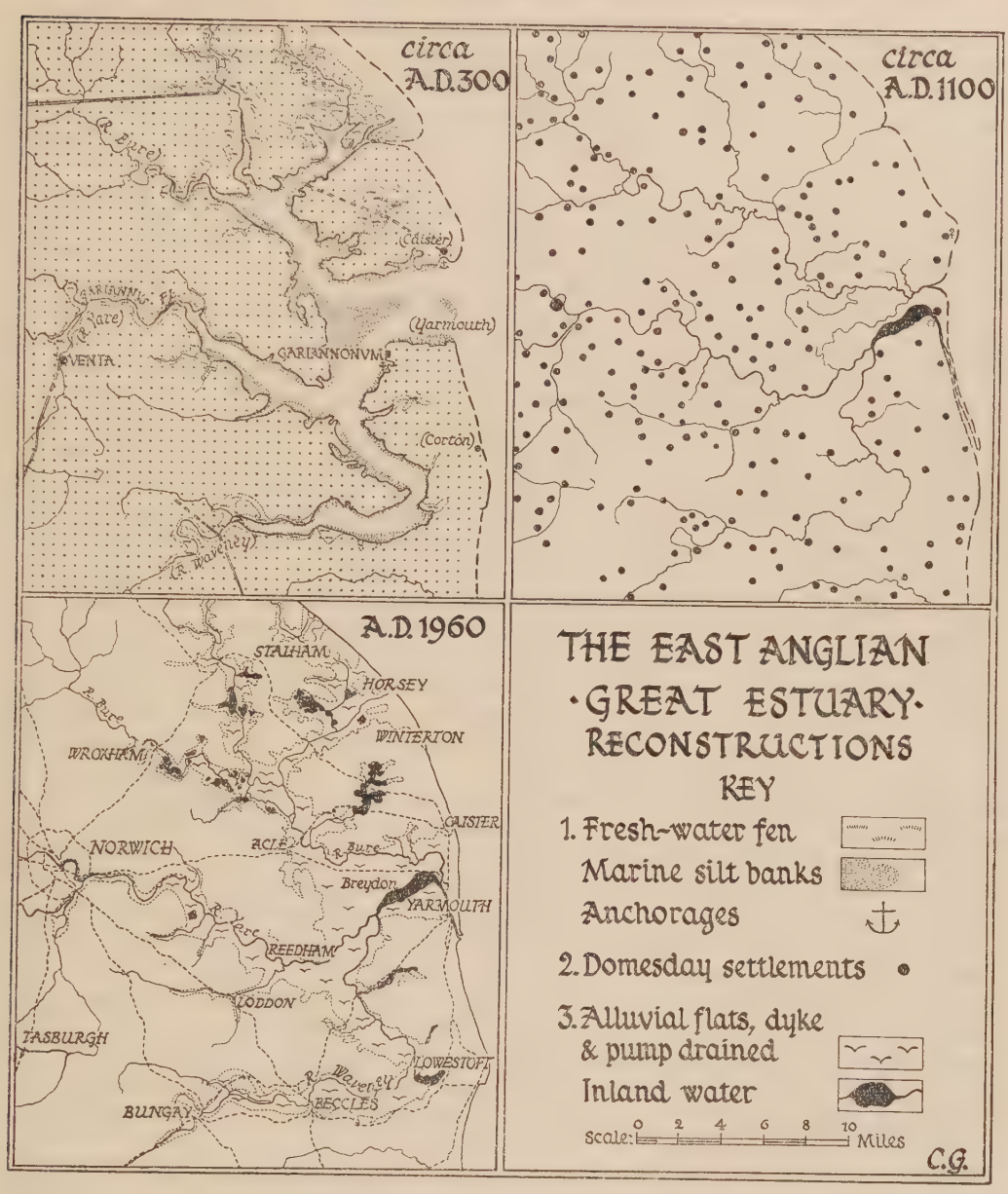

Fig. 1. (*Notes on the map reconstructions are on p. 27.*)

but this increases rapidly both to the north and the south until in the Humber there is a range of some 24 ft. and in the Thames some 25 ft. This is due to the fact—in accordance with the present-day theory of tide-formation—that the English coast at Yarmouth lies closest to the amphidromic point of the rotary stationary wave in the Flemish Bight which causes our southern North Sea tides (Doodson and Warburg 1941). The significance of

this is, of course, that O.D. levels well above high water in south-east Norfolk may be deeply covered in, e.g. the Thames, where Trinity High Water mark stands at 12·5 ft. O.D. There is also some evidence to suggest that North Sea tide ranges in the Middle Ages may have been a little higher than those of today, but the differences due to this fluctuation are probably not of such magnitude as greatly to modify the picture (Green and Hutchinson, 1960, 135).

We may now look at the evidence from the Wash and the Fenland. So much work has been done by the members of the Fenland Research Committee that the sequence of deposits is well established. To begin at the 'Buttery Clay': this is a layer which marks a marine transgression late in Neolithic times. This was followed by a peat-growth over the clay which Godwin has shown to be caused by a eustatic fall of sea-level. Then came the Iron Age marine transgression which raised great banks of silt at the head of the Wash (the 'Silt Fen') so that the more southerly 'Peat Fen' was cut off from all but exceptional flooding by the sea. A glance at the map of Roman Britain (O.S. 1956) shows that parts of the silt-fen were thickly occupied, and this we know began early in Romano–British times. Richmond (1955, 130) has indeed suggested that the rebellious Iceni, after their subjugation by Suetonius Paulinus, were constrained to work in the draining of this area. In the past, the so-called 'Roman Bank' which protected the silt-fen from the Wash was thought to belong to this period, though in later years much doubt has been thrown on this view. It is now known, however, that the 'Roman Bank' is of more than one build, and it seems probable that the primary bank does indeed date from early Roman times and formed part of the engineering activities which gave us the Cardyke and, a little later, the Peterborough —Denver causeway, which linked East Anglia with Ermine Street near Castor.[2]

Unpublished work in the silt-fen has, however, shown that late in the third century there was a rise in the water-table and settlement sites of the previous century now lay a foot or more below water.[3] The detailed work by Mrs Hallam (report forthcoming) confirms this, but tends to prove that this was due to freshwater rather than marine flooding, for the deserted earlier sites lay near the southern edge of the silt and were replaced by new sites closer to the sea.[4] Though the occurrence of marine 'surges' may also be involved, it is possible that a secondary oscillation may have in part been responsible for this slight submergence, an oscillation perhaps to be correlated with that suggested by Francis for the Essex shore of the Thames (*op. cit.*). No evidence of this, however, has yet been found at Yarmouth and, indeed, Godwin's borings at Wiggenhall, hard by Lynn, seem to show nothing of it. If then it can be substantiated, it can hardly have been more than temporary and the emergence continued here, as at Yarmouth for, by the time of Domesday, the pattern of silt-fen villages was very similar to that of today. It is true that when the distribution of Saxo-Norman pottery is mapped, the Fenland is virtually a blank area (e.g. Hurst, 1956, Fig. 1), but indeed the presence of Domesday settlements makes it certain that absence of pottery-records cannot be taken to mean that the area was unsuitable for domestic occupation.

Nevertheless, it is not easy to estimate the relationship of the land level to mean sea level at that time. The tide range in the Wash, e.g. at King's Lynn, is 18·5 ft., ranging between — 6·07 ft. and + 12·43 ft. O.D. This gives mean sea level as + 3·18 ft. O.D., higher than that at Yarmouth, which lies at — 0·77 ft. But to calculate from this and from present-

[2] A recent (1960) excavation by the writer at Denver, where the road emerges from the fen, has shown that the road here was in use at the beginning of the second century.

[3] I am grateful to Mr John Bromwich for this information and for his permission to use it.

[4] I am grateful to Dr and Mrs Hallam for discussing these points with me and for permission to refer to Mrs Hallam's forthcoming paper.

day land levels near the Wash-Fenland coast is misleading, for it is reasonably certain that considerable compression of underlying deposits has taken place, a compression which reduces land-level without change in that of the sea. This compression-factor is always difficult to assess but fortunately was not involved in the South Denes section at Yarmouth, for there the wave-packed granular structure of the spit, resting on Pleistocene sands, resists the pressures which modify the interdigitated peats and silts of the Fens. When, therefore, submergence began once again, further protection for the lower-lying areas would soon become necessary, and it seems probable that the later additions to the 'Roman Bank', which may by then have almost disappeared due to underlying compression,[5] began now to be made.

Much has been written on the losses to the sea by direct erosion of the Norfolk and Suffolk coasts. These, however, though considerable enough since late medieval times, may have been very little in the millennium before that. For it should be realized that losses of this kind tend to occur only during a submergence-phase. When emergence takes place, the beach-platform will come to stand above the sweep of the tide and generally become dune-covered, so that direct erosion by wave-action of the clay cliffs behind the dunes will cease and the cliff-face will soon be protected by weathering talus and blown sand. Not until submergence is again advanced and the beach-platform lies below high water, will the attack on the cliffs be renewed. From late Roman times to the 14th century, therefore, the coast-line was probably relatively stable. Supporting evidence for this may be found in Allison's (1955) survey of the lost villages of Norfolk. Of 130 villages listed, five only appear to have been lost—or probably lost—by coastal erosion. Four of these appear in Domesday, but one only of the four in the Nomina Villarum of 1316. The missing ones are Ness, Whimpwell and Shipden. Ness is known only by name, its exact location even being uncertain (but see MAP 2, FIG. 1), and it is impossible to estimate the date of its disappearance. Whimpwell, in Happisburgh, is commemorated by the name 'Whimpwell Green', though Allison does not consider that this marks the village site, which was lost before Blomefield's day (*ob.* 1751). It is, however, not certain, though it is probable enough, that the ancient Whimpwell now lies under the sea. The third, the port of Shipden, which stood two miles north of Cromer, disappeared in the days of Henry IV (1399–1413), when submergence was again in progress. The fourth village, Eccles, as is well known, began to suffer from marine attack at the end of the 16th century and the last cottages disappeared during the 19th. Its ruined church is visible even today, when the beach is storm-scoured. The fifth, Little Waxham, was mentioned neither in Domesday nor in the Nomina Villarum. Its rectors, however, were regularly appointed until 1383 and again in 1733. It is, therefore, not certain that an early village or hamlet was destroyed by erosion. If it was, the dates suggest that it may not have been until a century after submergence began once again. There is, therefore, no indisputable evidence of the loss of a single village by coast erosion between the 11th and the 14th centuries. As the eastern coastal region was the most thickly populated part of Norfolk and, indeed, of England, at the earlier date, it is evident that the changes brought about by the sea during those centuries were relatively negligible.

It was then in the 14th century, following the renewed submergence, that erosive action would begin again, to become more extensive in the post-medieval period. Of this, the disappearance of Dunwich and of the Saxon Shore fort at Walton may be cited as examples. Clearly at the latter there was but little loss after it was built until the later Middle Ages saw renewed attack on the coast. Again at Reculver we have the same story. The eating-

[5] Compression and the disappearance of artificially-raised banks is well-attested in Broadland.

away of the fort-area has been confined to the last few centuries after long-continued submergence had left it vulnerable. Broadly speaking, then, a reconstruction of the open coast-line at about A.D. 1400 will give us also a reasonably close picture of the same coast as it was a thousand years earlier, though this will not be true of the estuaries.

During the last century, most of the small ports of the East Anglian coast have become increasingly choked by silt-deposits, so that they are virtually 'dead' save for use by an occasional Thames barge and by local small fishing boats. This is largely due to the advent of power-driven craft and the increase in size of coasting vessels, which now tend to use the larger ports from which land transport disperses their cargoes. But the difficulties of navigating these small estuaries began to be felt before sailing craft had disappeared. It was, in fact, the approach of 'near-equilibrium' which brought this about. But the importance of these ports in this context lies in the fact that they first came into extensive use at a time when they would afford the most favourable conditions. When at the end of the 13th century the submergence began, the many small estuaries of the coast would be marked by deeply-cut rather narrow channels and, at first, submergence would tend to deepen these until the fresh deposition of marine silt filled them once again. It was at this time that seaborne trade, stimulated both by the activities of the Hanseatic League and by the growth of craftsmanship in the towns, began greatly to expand. The channel-conditions were at their best and coastwise seamen, who on this coast could compete very favourably with the primitive land transport from the greater ports, made the most of their advantages. A pattern of coastwise trade was thus able to develop which, in a few generations, became traditional and which continued in spite of growing difficulties until brought to an end by other factors.

It is probable, too, that the 'lagoon harbours', which in early historical times formed so prominent a feature of the Kent and Sussex coasts, owed their long survival to the Saxo-Norman emergence. The Wantsum, which divided Thanet from the Kentish mainland, provided a safe harbour which, in Roman times, was certainly one of the busiest then engaged in seaborne trade and which, in the days of the Saxon Shore, was the headquarters of the Classis Britannica. But here the problem of silting and the closure of the channel is greatly complicated by the presence of the Stonar shingle-bar and its outlying spit which, as Robinson and Cloet (1953) suggest, may have constricted the eastern entrance even in Roman times. At Pevensey, also, somewhat similar conditions must have obtained. Though now silt-choked, the old harbour of Anderida was still open to be used by William for his landing in 1066.

Presumptive evidence such as this outlined above is a pointer to the changes which appear to have occurred along the Saxon Shore in the last 2000 years. Further evidence may be afforded by the presence or absence of salt works at different times. The Essex 'Red Hills', for example, appear to have come to an end at the time of the maximum submergence (e.g. Reader *et al.*, 1908, 1910; Smith, 1918) and there is evidence of later occupation in Romano–British times. The gradients of rivers in this low-lying East Anglian area would be increased during the emergence and so stimulate the use of water-mills. As submergence progressed, such mills would become more difficult to work and many, operative at the time of Domesday, would no longer be of use at the end of the medieval period. Observations such as those in Kent by Evans (1953) and Jenkins (1954) must be integrated into the wider picture. But direct observation of stratigraphical relationships in deep cuttings, measured from Ordnance Datum, is required to substantiate them. Opportunity afforded by the building of a new power station at Yarmouth was taken by the writer, with the results outlined above. The erection of others at Bradwell in Essex and at Sizewell in Suffolk would seem to offer scope for corroborative work. For this,

however, a close acquaintance with local conditions is necessary, as well as the combined knowledge of archaeologist, naturalist and geologist, to interpret fully the interrelationships of natural phenomena and artifacts.

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NOTES ON THE MAP RECONSTRUCTIONS (Fig. 1, p. 23)

A.D 300. The estuarine coastline and the differentiation of the alluvium into tidal estuary and freshwater fen are based on direct evidence from borings and excavations. The precise extent of the estuary in the Horsey district is somewhat uncertain, but it is probable that a narrow channel to

the sea ran close to the Winterton shore. Similarly, the extent of the estuary close to Bungay on the Waveney is not fully known. It may perhaps have reached as far up-river as Bungay town.

The extent of the silt banks at this date is based largely on inference, though there is some direct evidence. They are probably somewhat conservatively restored and by this time many of the channels may have been rather narrower than they are shown. The open-sea coast is similarly based on inference from a variety of facts.

A.D. 1100. The open-sea coast is again based on inference. The promontory just to the north of Caister was almost certainly caused by marine deposition on an emergent beach platform and its remnants were to persist as 'Cuniver Point' until the first half of the 19th century. It is possible that this was the site of that 'Nessa' given in Domesday as in the Hundred of East Flegg. The location of this settlement has never satisfactorily been established for Blomefield (1805–10), the Norfolk historian, wrongly placed it at Winterton Ness in the Hundred of West Flegg and he has been followed uncritically by later writers, including Johnson and Salisbury (1906) in the Victoria County History.

I am grateful to Dr J. M. Lambert for much help in the plotting of the estuarine deposits.

Book Chronicle—*continued from p. 20.*

THE KINGDOM OF CHRIST. *By* PETER BAMM. *pp.* 368, 265 *illustrations*, 18 *colour plates. Thames and Hudson, London*, 1959, 42*s.* Dr Bamm gives almost as much weight to historical fiction as he does to historical fact. His book lacks real authority but as a widely travelled man, he is more successful in capturing the atmosphere of the diverse regions which he describes—the excellent illustrations are well chosen from this point of view. Despite its obvious defects as a book of reference it has one outstanding merit—the sincerity of its author and his unquestioning faith in the truths of the Christian religion. The *apologia* at the end of the book is a fine piece of writing. The convinced Christian, in fact, may find much to charm him and increase his knowledge of religious lore; but Dr Bamm does not write for the sceptic.

THE ETRUSCAN LION. *By* W. LLEWELLYN BROWN. *pp.* xxvi, 209, 64 *plates*, 1 *map. Oxford, The Clarendon Press*, 1960. £4 4*s.* An Oxford Monograph on Classical Archaeology prepared for publication by John Boardman after the author's death in 1958.

PREHISTORIC INVESTIGATIONS IN IRAQI KURDISTAN. *By* ROBERT J. BRAIDWOOD *and* BRUCE HOWE. *pp.* 184, 29 *plates. The University of Chicago Press*, 1960. 5 *dollars.* Published by the Oriental Institute of Chicago as no. 31 in their series 'Studies in Ancient Oriental Civilisation'.

ST NINIAN'S ISLE TREASURE. 6 *pp. of text by* ANDREW O'DELL *and* 40 *pages of photographs taken by* ALEXANDER CAIN. *Published for the University of Aberdeen by Oliver and Boyd, Edinburgh*, 1960. 10*s.* (Aberdeen University Studies, no. 141.)

BOARD AND TABLE GAMES. *By* R. C. BELL. *pp.* xxiv, 208, 24 *plates, many text figures. Oxford University Press*, 1960. 21*s.*

THE SCOTTISH CASTLE. *By* STEWART CRUDEN. *pp.* xv, 272, 48 *plates*, 25 *text figures. Nelson, Edinburgh*, 1960. 42*s.*

[*continued on p.* 36.

ANTIQUITY XXXV, 1961

Civitas—A Myth ?

by SHEPPARD FRERE

In the September number of ANTIQUITY (1960, 222) *Mr J. C. Mann took Mr Sheppard Frere to task over the definition of the Gallic or Romano-British* civitas. *Here Mr Frere, Reader in the Archaeology of the Roman Provinces in the University of London, discusses this controversial issue.*

MR J. C. MANN holds that 'each *civitas* of Gaul (and of Britain) was presumably a city, and like all true cities, each will have consisted of a single physical city surrounded by its *territorium*, the whole under the control of its magistrates, council and assembly, the last no doubt usually of little importance.' I, on the other hand, following Jullian and Stevens, have suggested that the *civitas* was the whole tribal area, its capital (*caput*) normally ranking merely as a *vicus* where the government happened to have its seat. This, Mr Mann suggests, is a myth, which he doubts whether a Roman lawyer would understand.

The evidence admittedly is not all that it might be; yet as the question is important for a correct understanding of the development of local government in Roman Britain (and elsewhere), I may venture to take it up once more, especially as Mr Mann's note appears to me to be erroneous in one or two particulars, and to exhibit the triple weaknesses of a too narrowly legalistic outlook, an inadequate account of historical development, and a neglect of some of the relevant facts.

He makes out the Romans to be a good deal more hide-bound in their institutions than history shows us to be the case. The Conquest of the West posed new problems of organization, to which it was wisest to find new solutions. The tribes of Narbonensis were still sufficiently small to be treated as city-states, or became so when the foundations of Caesarian *coloniae* partitioned their territory. But north of this the tribal areas were so vast as to make such treatment ludicrously out-of-scale. Roman administration, however, could adapt and experiment. In Spain we find the *conventus* system tried out, while in Gaul and Britain, and elsewhere too, the *civitates* were organized. That eventually *civitas* as a legal term came to mean a self-governing town is no doubt true, but only at a date when it no longer meant a non-Roman community; it is equally true, however, that the organization given to the *civitates* though based for the most part on that of official foundations, had a very different character and much variety. Indeed it was in this power to give a changed content to the same legal formula that we can recognize the genius of Roman practical government. In what follows, therefore, I shall attempt (*a*) to examine the meaning of *civitas* and trace its development, (*b*) to consider what evidence we possess for the status of the tribal capital (*caput*), (*c*) to show that wide differences existed between the organization of the *civitates* and the municipal constitutions of 'normal' Roman cities, sufficiently wide indeed to make it dangerous to regard them merely as the lower grades of a uniform municipal hierarchy.

A. THE DEVELOPMENT OF THE TERM CIVITAS

Mr Mann writes '*Civitas* has two meanings as a legal term: (1) a self-governing city, the

exact equivalent of the Greek πόλις: (2) citizenship, as in *civitas Romana. . . .*' But it is a mistake to take the lawyer's point of view without combining it with that of the historian. Institutions develop: technical terms must be related to the march of history. One wonders what Mr Mann's Roman lawyer would have made of the continual references of Caesar to Gallic and even British *civitates*.[1] The meaning and development of the term was succinctly and accurately summarized by Kornemann in 1903[2]: (1) The primary meaning is 'the totality of citizens, the citizen body (die Geshamtheit der *cives*, die Bürgerschaft). (2) Later by extension it comes to denote the town (übertragen wird das Wort dann auch . . . auf . . . die Stadt, . . . = *oppidum*, *urbs*). (3) It also means citizenship.

The second meaning, *civitas* as town, is first clearly employed by Vitruvius, writing under Augustus: *civitatis amplissimae moenia*.[3] Seneca can use it of the *colonia* of Lyons and Tacitus of Rome itself.[4] The primary meaning appears earlier. Cicero in one passage expressly distinguishes it from *urbs*.[5] Caesar can say *omnis civitas Helvetia in quattuor pagos divisa est*.[6] This, then, is the first stage we need note: the Caesarian usage stands for a tribe or tribal area, a self-governing community, not yet a self-governing town.

The second stage current from the first down to at least the early 3rd century A.D. is a twofold one, referring sometimes to the town, sometimes to the tribal territory, or rather to the people or community therein. First Frontinus[7]: *habent autem provinciae et municipales agros aut civitatium peregrinarum:* provincial land can be classified under various headings, one of which is *civitas peregrina*. These *civitates peregrinae* are the non-Roman self-governing communities, and are distinct from municipal or colonial territory. In Mann's view *civitas peregrina* has only one meaning, that of πόλις. But this is mistaken and over-simplified. In Tacitus two distinct usages are frequently found. For instance in *Agricola* 20 he is clearly referring to the tribal area and its inhabitants: *multae civitates quae in illum diem ex aequo egerant datis obsidibus iram posuere, et praesidiis castellisque circumdatae . . .*; whereas in *Histories*, IV, 65 *muros civitatis* refers to the walls of Cologne.[8] Even as late as *c.* A.D. 210–25 the jurist Ulpian can use the word in these two different senses.[9]

[1] Cf. *BG*, v, 12; *Britanniae . . . incolitur . . . maritima pars ab eis qui . . . ex Belgio transierant qui omnes fere eis nominibus civitatum appellantur quibus orti ex civitatibus eo pervenerunt.* Or *ibid*, v, 20; *Trinobantes prope firmissima earum regionum civitas . . .*

[2] Pauly-Wissowa, *RE*, Suppl. I, 300–17.

[3] Vitruvius, 32, 1; cf. 19, 9; 23, 21, etc.

[4] Seneca, *Epist. Mor.*, XCI, 10, describing the great fire there in A.D. 65; *civitas arsit opulenta.* Tac., *Hist.* I, 19; II, 92; IV, 2. This usage is admittedly the equivalent of πόλις, but it has little relevance to the question of the provincial *civitates* of the West.

[5] *pro Sestio*, 42, 91. He is tracing the evolution of civic progress. . . . *quodam tempore homines nondum neque naturali neque civili iure descripto fusi per agros ac dispersi vagarentur . . . Tum res ad communem utilitatem quas publicas appellamus, tum conventicula hominum, quae postea civitates nominatae sunt, tum domicilia coniuncta, quas urbis dicimus, invento et divino iure et humano moenibus saepserunt.*

Contrast also *de Repub.*, 26, 41; *coniunctionem tectorum oppidum vel urbem appellaverunt* with *ibid.*, 25, 40, *quid est autem civitas nisi multitudo hominum in quoddam vinculum redacta concordiae.*

[6] *BG*, I, 12.

[7] Writing under Domitian: Lachmann, *Gromatici veteres*, I (1848), 35–6. The whole passage is notable at its date and deserves quotation. *Prima enim condicio possidendi haec est ac per Italiam; ubi nullus ager est tributarius sed aut colonicus aut municipalis aut alicuius castelli aut conciliabuli aut saltus privati. At si ad provincias respiciamus, habent agros colonicos eiusdem iuris, habent et colonicos qui sunt immunes, habent et colonicos stipendiarios: habent autem provinciae et municipales agros aut civitatium peregrinarum.*

[8] Contrast also, e.g., *Hist.* IV, 70; *legiones in Mediomatricos sociam civitatem abscessere* (the tribal area) with *ibid.* I, 63, *Divoduri* (*Mediomatricorum id oppidum est*) *quamquam omni comitate exceptos subitus pavor terruit, raptis repente armis ad caedem innoxiae civitatis . . .* (the town); or *Ann.* III, 41, *haud ferme ulla civitas intacta seminibus eius motus fuit, sed erupere primi Andecavi ac Turoni* (a tribal rebellion) with *Hist.* I, 54, *miserat civitas Lingonum vetere instituto dona legionibus* (the tribal government).

[9] *Digest*, I, viii, 9. *sacra loca ea sunt quae publice sunt dedicata, sive in civitate sunt sive in agro*, where the

The inscriptions, on the other hand, show us *civitas* as equivalent to the tribal government (the *ordo*),[10] as the mouthpiece of the people; and this is what Cicero meant when he wrote *omnis civitas quae est constitutio populi*, distinguishing it from *populus* on the one hand and *respublica* on the other.[11]

It is clear that the use of *civitas* in the authorities as the equivalent of πόλις, if indeed ever identical where the reference is to a *civitas peregrina*, dates from after the political progress made under the early Empire. By degrees the self-governing communities became more and more urbanized. Ammianus Marcellinus in the fourth century seems to use the word mainly of towns, but perhaps sometimes with a slightly distinct shade of meaning as town plus surrounding community. In Book XVI, ii, 1, he uses it of the city of Autun: *comperit Augustoduni civitatis antiquae muros . . . invalidos*; in XVI, ii, 12 the meaning is town and surrounding district: *audiens itaque Argentoratum Brotomagum Tabernas Salisonem . . . et Moguntiacum civitates barbaros possidentes territoria earum habitare—nam ipsa oppida . . . declinant— . . . primam omnium Brotomagum occupavit*[12] (the actual towns are unoccupied). In 4th-century Imperial edicts the usage is as city or city government.[13]

We have other evidence that there was a strong centripetal tendency in the *civitates* leading to the glorification of the town, seen for instance in the well-known development of, e.g. *Durocortorum Remorum* into plain *Remi*, so that by the 4th century, for example, Ammianus can write *profectus itaque ab Ambianis Treverosque festinans* (XXVII, viii, 1) of a journey from Amiens to Trier[14]: at this final stage we can agree with Mann that there was virtual identity of *civitas* and capital. But it came about slowly. It represented the ultimate triumph of the Roman policy of urbanization applied to such new conditions.

To sum up: in the early Empire *civitas* can be used of towns of all sorts. But where the reference is explicitly or implicitly to *civitates peregrinae* the meaning is usually found to be the whole community in its tribal territory and/or its government. It was only with the slow climb of the cities to primacy in the tribal areas that the meaning of *civitas* came to be equated with πόλις: and this equation is not to be placed before the 3rd century A.D., with its full establishment in the 4th. In this development the Edict of Caracalla marks a central decisive point, for now all would require a town as *origo*. But it did not mark the end of the development, as the quotations cited from contemporary jurists show; time was still required for the full effects to work themselves out.

B. THE STATUS OF THE TRIBAL CAPITAL

The jurist Paul, writing in the early 3rd century on the proving of wills, gives us a list of recognized communities, in which the word *civitas* does not figure. *Testamenta, in municipiis coloniis oppidis praefectura vico castello conciliabulo facta, in foro vel basilica praesentibus*

contrast is between town and country; *ibid.*, L, XV, 4, *forma censuali cavetur ut agri sic in censum referantur. Nomen fundi cuiusque: et in qua civitate et in quo pago sit*, where on the tax-return the address of each farm must be given, both its district and 'county'. The two usages are distinct, and are closely parallel to those found in Tacitus, despite the fact that Ulpian is in all likelihood writing after the *Constitutio Antoniniana*.

[10] See note 35.

[11] *de Repub.*, I, 26, 41.

[12] He uses the word *oppidum*, not *civitas*, in describing the cities of Gaul recovered by Julian, XX, iv, 1 (cf. XVI, iii, 3). For Britain compare XXVIII, iii, 2, *in integrum restituit civitates et castra*, with XXVIII, iii, 7, *instaurabat urbes et praesidaria* (*ut diximus*) *castra* where the civilian programmes are not explicitly identical.

[13] *Codex Theod.*, I, 29, 6; XII, 1, 12, etc.

[14] The earliest such usages appear on the milestones of the third century, e.g. C.I.L., XIII, 8953 Rennes, *A C*(*ivitate*) *R*(*edonum*) *L*(*eugae*), A.D. 237; *ibid.* 8911 Limoges, *L*(*emovicibus*) XXXIII, A.D. 243; 9040, Rheims, *C*(*ivitate*) *Rem*(*orum*) *L*(*eugae*) IIII, AD. 265–70, etc. It is clear that Jullian was correct (*Histoire de la Gaule*, IV, 525 ff.) in seeing here the result of the Edict of Caracalla, whereby Roman citizenship was extended to all free inhabitants of the Empire, with consequent changes in the standing and significance of the towns.

testibus vel honestis viris inter horam secundam et decimam diei aperiri recitarique debebunt . . .[15] Here the term which includes the *caput* of a former peregrine *civitas* is *oppidum*, *vicus* being used to designate the rural settlements of the *civitas*. But the word *oppidum* lacked legal precision, and in status such *oppida*, as we shall see, themselves ranked normally also as *vici*. That this was so seems to follow from Strabo's account of Vienne and the Allobroges. He is describing a time before the city became a *colonia* which occurred not long before 27 B.C.[16] He writes[17]: 'Formerly the Allobroges kept up warfare with many myriads of men, but now they cultivate the plains and the glens in the Alps, and all of them live in villages, except that the most notable of them dwelling in Vienne (formerly itself a village but called nevertheless the metropolis of the tribe) have built it up into a city.' In this case, then, Vienne, the *caput* of the Allobroges, ranked as a *vicus*, but was nevertheless their metropolis or *chef-lieu*.

In the early years it was felt to be important to preserve the strict legal distinctions between communities, so that for instance when the *colonia* of Lugdunum was founded in 43 B.C. the Segusiavi, in whose territory it lay, were provided with a new *caput* of their own, Forum Segusiavorum.[18] But when in due course Forum Segusiavorum itself under the Flavians was granted promotion to colonial rank the empire was beginning to outgrow these rigid distinctions[19]; and this may be the explanation of I.L.S. 1020 (quoted on p. 35).[20] It is also evidently the explanation of Frontinus's provincial *agros colonicos stipendiarios*.[21] We must then be prepared to find that in course of time some *oppida*, capitals of *civitates*, were granted higher status as *municipia* or *coloniae*, but continued nevertheless as *capita civitatis*. This, no doubt, is what happened at Verulamium and at Leicester (if Leicester is accepted as a municipium),[22] as it did at Trier. Such elevation was, however, both exceptional and the gift of time.[23] That the normal status of a peregrine town was that of *vicus* seems clear not only from the passage from Strabo quoted above but also from the inscription from Sens (I.L.S. 7049) which Mann himself quotes. Here C. Amatius Paternus is described as *aedil(i) vikan(orum) Agied(incensium)*, *aedil(i) c(ivitatis) S(enonum)*, aedile of the *vicani* of Sens, aedile of the *civitas* of the Senones. Mr. Mann says: 'the use of this word [vicani] need occasion no . . . surprise . . . it cannot be held to prove that Agedincum was merely a *vicus* in the *civitas Senonum*.' One may be excused for profoundly disagreeing. In the first place, the juxtaposition of the two phrases, if it proves nothing more, proves a difference in organization to exist between the *civitas* of the Senones on the one hand and the town of Agedincum on the other. Agedincum was *not* the *civitas Senonum*.

[15] IV, vi, 2. It is interesting to note the differences in this passage from its late Republican analogue in the *Lex Iulia Municipalis* (I.L.S. 6085, *passim*): *municipiis, coloniis, praefecturis, foris, conciliabulis civium Romanorum*. The latter is designed for communities of Roman citizens, the former adapted for the altered requirements of the Empire, namely the inclusion of the peregrine communities of the provinces.

[16] Jullian, *op. cit.*, IV, 32; Grenier, *Manuel d'Archéologie Gallo-Romaine*, I, 323; Hatt, *Histoire de la Gaule Romaine*, 91.

[17] IV, i, 11. *Ἀλλόβριγες δὲ μυριάσι πολλαῖς πρότερον μὲν ἐστράτευον, νῦν δὲ γεωργοῦσι τὰ πεδία καὶ τοὺς αὐλῶνας τοὺς ἐν ταῖς Ἄλπεσι, καὶ οἱ μὲν ἄλλοι κωμηδὸν ζῶσιν, οἱ δ'ἐπιφανέστατοι τὴν Οὐίεννναν ἔχοντες, κώμην πρότερον οὖσαν, μητρόπολιν δ'ὅμως τοῦ ἔθνους λεγομένην, κατεσκευάκασι πόλιν.*

The word *κώμη* is exactly equivalent to *vicus*.

[18] C.I.L., XIII, 221.

[19] On Forum Segusiavorum see also Vittinghoff, Römische Stadtrechtsformen zur Kaiserzeit, *Zeitschrift der Savigny-Stiftung für Rechtsgeschichte, Romanistische Abteilung*, (81) LXVIII (1951), 483.

[20] On the difficulties of this see Vittinghoff, *op. cit.* (n. 19 above), p. 475, note 131.

[21] See footnote 7 above.

[22] Rivet, *Town and Country in R.B.*, 65; J. E. Bogaers, *Civitas en Stad van de Bataven en Canninefaten* (Nijmegen, 1960), p. 18 ff. and note 225.

[23] For the date and number of such *coloniae* in Gaul see Jullian, *op. cit.*, IV, 261–2.

And, of course, this difference between capital and *civitas* is implicit in other inscriptions. The famous pedestal[24] at Caerwent was set up by the *respublica civitatis Silurum*: no mention of *Venta Silurum*, Caerwent itself. If, later on, the capitals came to drop their first in favour of their second, the tribal, name (thereby as we have seen drawing attention to the primacy of the city), yet in this very change once more we see the distinction from the 'normal' Roman town, and can recognize the consummation of that experiment in governmental organization mentioned earlier. The *civitas* has crystallized in its capital: the capital has not engulfed the *civitas*.

But to return to Mr Mann. Secondly, 'the *vicani* of Agedincum,' he writes, 'are merely the inhabitants of all or some of the wards of the city', and goes on to show that the colony of Ariminum on the Adriatic coast of Italy consisted of seven such *vici*.[25] But this is begging the question. Can Mr Mann show any peregrine capital in Gaul or Britain whose *constituent vici* elected aediles? Constituent *vici* are, of course, attested even in Gallic tribal capitals.[26] But *Vikan. Agied.* in this context cannot mean inhabitants of *some* of the wards of Sens, for an aedile of some of the wards would have to be differentiated from an aedile of others. It must mean either the inhabitants of all the *vici* of Agedincum or (much more probably) the inhabitants of the *vicus* of Agedincum.[27] In either case the aedile represents the inhabitants of Agedincum: he is the aedile of the town, and is junior to the aedile of the *civitas*.

Now we have in Britain, of course, the very interesting inscription from Brough on Humber (Petuaria) which is relevant here, and Mr Mann does not forget it. 'It now seems probable that the *vicus* attested at Brough . . . has nothing to do with the organization of any city which may have been founded here.' The reverse is, I think, the case, and the known historical facts knit together so neatly that it is difficult to regard the opposite conclusion as other than perverse. The inscription mentions an *aedilis vici Petu(arensium)* and dates from the reign of Antoninus Pius.[28] What are the other facts? John Wacher's excavations have shown that the civilian town was being developed *on the site of the fort* during the second quarter of the second century.[29] Like many other towns further south in Britain, Petuaria had its origins in a military vicus. But to say 'so long as it was a *vicus* under military control (as *vici* in the military area normally were), it has no relevance to city government' is once more to beg the question. For the archaeological evidence goes to show that this military control was removed when the site of the fort was redeveloped as the site of the town[30] in the second quarter of the 2nd century: and this converges nicely with the date of the

[24] *Arch.*, LIX, 120; *Ephem. Epigr.*, IX, 1012; Haverfield, *Romanization of Roman Britain*, 59; id., *Roman Occupation of Britain*, 185. Cf. also the Wroxeter Forum inscription dedicated by the Civitas Cornoviorum. Atkinson, *Excavations at Wroxeter* 1923–7, Frontispiece and p. 177; *JRS*, XIV, 244; Richmond, *Roman Britain* (Britain in Pictures), 23. In both these cases it is the whole community, not the town alone, at work through its government. The formula of the Caerwent stone, though 3rd century in date, is parallel to that of Wroxeter under Hadrian, and this despite the fact that in Britain the tribal names as far as we know were not transferred to the towns to anything like the same extent (if at all) as in Gaul (p. 31). There would have been nothing at this date to inhibit the use of the name Venta in the formula except its non-correspondence with the political reality described.

[25] Grenier long ago made this point 'une ville qui n'est pas colonie n'est elle-meme qu'un vicus; un quartier, une rue, un faubourg de cette ville est également un vicus', *Manuel d'Archéologie*, II, ii, 695; cf. Pauly-Wissowa s.v. Vicus 2091–2.

[26] Metz, C.I.L., XIII, 4301 (= I.L.S. 4818), possibly Nantes; *ibid.* 3106 (= I.L.S. 7051).

[27] cf. the opinion of Hirschfeld, C.I.L., XIII, p. 443.

[28] See Birley, *Trans. E. Riding Antiquarian Soc.*, XXVIII, iii (reprinted as *Hull Museum Publ.* 206), pp. 229–32; also more shortly in *JBAA*, VII (1942), 27–8; *JRS*, XXVIII (1938), 199.

[29] *Antiq. Journ.*, XL, 61–2; cf. *JBAA*, VII (1942), 8–13.

[30] The early military *vicus* must have been elsewhere.

inscription with its testimony to a civilian official providing an amenity for the new town at his own expense (in the form of a stage for the theatre). This official was *aedilis vici Petuarensium*. There can be no question of his being an aedile of a *vicus in* Petuaria, even if the size of the town (*c.* 12 acres within the walls) had allowed us to conceive of constituent *vici* here. He is aedile of Petuaria the *vicus*, not, be it noted, aedile of the *civitas Parisorum*. The parallel with Agedincum is very close. It remains to consider whether Petuaria was the capital of the Parisi. There can be no certainty about this, but a high degree of probability. Petuaria is the only walled town known in the tribal area, whatever may be the significance of its etymology, and is the only πόλις ascribed by Ptolemy to the tribe. Unfortunately Ptolemy's use of this word is loose, since he applies it equally to Roman forts and even native hill-forts, so that no deductions can be based on πόλις as a technical term. Nevertheless as the sole site mentioned among the Parisi (and the sole walled town known there) it has a strong claim to be the *caput*. If so the *caput* ranked legally as a *vicus*, as did Agedincum and Vienne.

C. THE DIFFERENCES BETWEEN CIVITATES AND OFFICIAL FOUNDATIONS

Perhaps the most disturbing sentence in Mann's note is his statement 'The *civitates* differed only in status from Roman colonies and *municipia*'. This is indeed the kernel of his argument, and it is the crux of the matter. In my review of Rivet's *Town and Country in Roman Britain*[31] I claimed that the tribal capital 'was no more than a *vicus* in the *civitas*', in other words that the essence of the peregrine *civitas* was not the town itself but the whole tribal area (the *chef-lieu* being but the repository of government without special status of its own): that in fact the peregrine *civitas* was a *different kind* of political organization from the colony or *municipium*.[32]

It is, of course, not yet possible to give a complete account of the organization of the peregrine *civitates* either in Gaul or in Britain as Mann himself remarks; but it will be sufficient to show that they differed in kind as well as in degree from the Roman municipal pattern to show the error of the sentence I have quoted.

The organization of Gaul though due in detail to the work of Agrippa and Augustus, rested ultimately on the arrangements of Caesar. Not a few of the Gallic tribes were *civitates foederatae* (that is, they had willingly entered into a treaty-relationship with Rome), others were *liberae et immunes* (they had received a grant of 'liberty' and 'immunity from taxation' from their conquerors). The rest were *stipendiariae*. The exact legal distinctions between these categories became blurred with time; but though in one sense the differences were smoothed out by a levelling downwards, in that all paid taxes and all were subject to the provincial governors, in another sense it was a levelling upwards; the conquered Nervii or Armorican tribes had exactly the same order of tribal government as did the allied Remi or Aedui. In other words the allied status of these tribes preserved their loose organization through the formative stage of Roman Gaul, and the other tribes (who might have been reorganized) are found to be likewise. That no uniform municipal pattern was imposed is clear also from the survival of local pre-Roman titles such as Vergobret or Gutuater.[33] All that Rome did was to encourage the building of a suitable *chef-lieu* for the seat of government, and adapt the local aristocratic constitution to the Roman model. This took time.[34]

[31] ANTIQUITY (1959), 67–8: see also *Past and Present*, 16 (November, 1959), 4.

[32] Of the organization of the latter a convenient account will be found in Stevenson, *Roman Provincial Administration*, Oxford 1939, ch. VI.

[33] C.I.L., XIII, 1048, 2585 (=I.L.S. 7040, 7045).

[34] Cf. Jullian, *Histoire de la Gaule*, IV, 336–8.

The *civitas* was the tribe and its government, not the new city.[35] Indeed it could hardly be otherwise since the new towns did not spring up overnight. Some were still providing *fora* as late as the Flavian period or later.[36]

That there was indeed a difference in kind between the *civitates* and the Roman pattern has been made clear by the inscription from Sens (I.L.S. 7049) discussed above. But the case for differences in kind between the *civitates peregrinae* and the organization of colonies and *municipia* with their *territoria* is stronger than this.

(*a*) The enormous difference in size has already been mentioned, and many Gallic and some British *civitates* contained sizeable towns in addition to the *caput* (some of them with indications of a certain measure of individual organization, e.g. C.I.L. XIII, 3067, a *curator Cenabensium* at Orleans). Furthermore some small tribes were incorporated with others[37] to form a single *civitas*—curious if the smaller unity was the desideratum.

(*b*) The inscriptions sometimes show us a strange variety of terms (e.g. I.L.S. 1020 *Colonia Pia Flavia Constans Emerita Helvetiorum foederata* at Aventicum) or of titles (I.L.S. 7042, *praefectus colon(iae?)* at Le Puy; C.I.L. XIII, 1357, *praefectus Iuliensium* at Vaison; XIII, 8771, *summus magistratus civitatis Batavorum* at Ruimel) such as would seriously shock Mr Mann's Roman Lawyer.

(*c*) The set-up of the *civitates* in many cases was arranged by negotiation both in Gaul (as we have seen) and in Britain,[38] and we hear that all the treaties (*συμβάσεις*) which Claudius or his legates might make were voted an advance validity by the Senate, whose prerogative the supervision of these agreements still evidently was.[39] While lack of information about the precise treaty terms prevents us going into details, one may legitimately doubt whether tribes voluntarily entering agreements would be required to undergo a wholly radical and alien transformation of their constitutions, especially in view of what is known of Claudius's character and, e.g., the highly individual arrangements made with Cogidubnus.[40] We should expect latitude and regional variations, and this is confirmed for instance by what we know of the constitution of the Petrucorii,[41] or by the fact that vergobrets continued to be elected, single chief magistrates instead of the usual pair.

(*d*) Even between the *civitates peregrinae* of various provinces there were serious differences in organization as well as in stages of self-government, as has lately been shown for Pannonia by Mócsy.[42] These differences and stages can only be understood as the working of practical Roman statecraft applying itself to situations as it found them, untrammelled by legalistic traditions, but nevertheless by early compromise achieving final success in the

[35] Cf. I.L.S. 7041, *Marco Lucterio . . . civitas Cadurcorum . . . publice posuit*; 7043, 7044; 7045, *C. Sulpicii Galli . . . cui ordo . . . statuas publice ponendas decrevit.*

[36] ANTIQUITY (1947) 75; cf. also the case of Evreux, *Gallia*, I, ii, 192. For the as yet incompletely Romanized social structure of Gaul in the first century see Syme, *Tacitus*, Oxford 1958, 456–61.

[37] Jullian, *op. cit.*, IV, 71: for the general question see also 326.

[38] Dio, LX, 22 (*τοὺς μὲν ὁμολογίᾳ, τοὺς δὲ καὶ βίᾳ προσαγαγόμενος*).

[39] *ibid.*, 23.

[40] Is it possible that a tradition of this state of affairs in his sources underlies Dio's repeated statement that *two* cities (in LXII, 7, 1 he calls them '*Ρωμαϊκὰς πόλεις*, *Roman* cities) were destroyed by Boudicca? These must be the *colonia* of Camulodunum and the *municipium* of Verulamium: no mention of London, then doubtless only a *vicus*. This is so notably different from the account of Tacitus (*Ann.*, XIV, 37) who emphasizes both London's richness and her fate, that it calls for comment. Dio is restricting the word *πόλις* to the official foundations.

[41] C. E. Stevens in J. M. Wallace-Hadrill and J. McManners, *France: Government and Society* (1957), pp. 22–3, where he shows that the 'four kinship groups' (*coriae*) of the pre-Roman tribe were perpetuated in the four archdeaconries, with a fifth at Périgueux itself, into which the Roman church, basing itself on the civil administration, divided the diocese.

[42] Zur Geschichte der peregrinen Gemeinden in Pannonien, *Historia*, VI (1957), 490, 492, etc.

implantation of urban traditions. Variety rather than uniformity is what we find, and there is nothing irregular in the conception of the *civitas* transcending its *caput*, or in the suggestion that the *caput* itself ranked normally as a *vicus*, at least as long as such technicalities retained any significance.

Book Chronicle—*continued from p. 28.*

A CONCISE ENCYCLOPAEDIA OF ARCHAEOLOGY. *Edited by* LEONARD COTTRELL. *pp.* 512, 16 *colour plates*, 160 *monochrome plates* (*including ten line maps printed on art paper*). *London, Hutchinson*, 1960. 50*s.* The newest volume in the *New Horizons Books* series of which some volumes already published are so good (see ANTIQUITY, 1959, 75). By no stretch of any reasonable archaeological reviewer's imagination could this be described as good. It has many good things, as any list of contributors which includes Desmond Clark, Sonia Cole, Adrian Digby, John D. Evans, C. J. Gadd, J. Edward Kidder Jr, Louis Leakey, Charles McBurney, Margaret Murray, Kenneth Oakley, and Lord William Taylour—to mention a few—ensures. But even twenty swallows do not make a spring or summer, nor twenty woodcock a winter. This book has fallen between all the available stools and is not to be recommended to any except learned archaeologists who can distinguish between the good entries and the bad, and who can bring themselves to use it for fun as a quarry for ghastly howlers and egregious mistakes. The list of obvious omissions would take pages of ANTIQUITY, but it really is sad that New Grange, Montelius, Worsaae, Los Millares, Mycenae, Hadrian's Wall, Treasure Trove, Antequera, Resistivity-Survey and Archaeo-magnetism—to mention ten non-existent entries at random—should be absent from what is not, we are afraid, a reliable encyclopaedia of archaeology. Was there anything so silly printed as the entry under *Dolmen*, 'This name and cromlech originally had the same meaning—a megalithic tomb of Neolithic times. Now this meaning is reserved for dolmen'. *Entendu;* but what does this reserved meaning mean? This book is a sad object-lesson in the dangers of popular archaeology.

JOURNAL OF ROMAN STUDIES. Vol. L (1960), *pp.* 314, 26 *plates*, 35 *text figures.* The Jubilee Volume published by the Society for the Promotion of Roman Studies, London, 1960. Price to non-members of the Society (obtainable from booksellers), £3.

THE MAKING OF MAN. *By* I. W. CORNWALL and M. MAITLAND HOWARD. *pp* 63, 61 *text figures*, 2 *tables. London, Phoenix House*, 1960. 10*s.* 6*d.* A brief clear account of the physical evolution of man and the fossil evidence for it. The text by Ian Cornwall, the illustrations (often in two colours) by M. Maitland Howard. A very successful example of text and illustrations neatly wedded together.

LE TRAVAIL DU METAL AU PAYS DE SUMER AU TEMPS DE LA IIIe DYNASTIE D'UR. *By* HENRI LINET. (Vol. clv of the *Bibliothèque de la Faculté de Philosophie et Lettres de L'Université de Liège.*) *pp.* 332, 20 *text figures. Paris*, 1960. *Société d'Edition 'Les Belles Lettres'*, 95 *Boulevard Raspail, no price.*

THE EARLY CHARTERS OF THE WEST MIDLANDS. *By* H. P. R. FINBERG. *pp.* 256. *Leicester, The University Press*, 1961. 50*s.* The second volume in the Leicester Series of *Studies in Early English History*, of which the first was Eric John's *Land Tenure in Early England.*

[*continued on p.* 39.

Australian Radio-carbon Dates

by D. J. MULVANEY

Mr Mulvaney is a Lecturer in Ancient History at Melbourne University and has for many years been concerned with the prehistory of Australia. Here he discusses the significance of some of the first Australian C14 dates. Originally quoted as dates in years 'Before the Present' they have been converted into dates B.C. or A.D. as this is at present the common usage in Britain.

THE Antipodean archaeologist has more than passing interest in the opinions expressed in ANTIQUITY concerning the reliability of radio-carbon 14 age estimations. To him, objective prehistory seems unattainable without the combination of stratigraphy and radio-carbon. Other geochronological schemes remain to be devised and tested in Australia, while the material simplicity of aboriginal culture deprives the prehistorian of many customary ancillary aids, such as coinage, pottery or sophisticated typology. Even the ubiquitous faïence bead has proved elusive. In the Polynesian world there is dispute concerning the value of genealogies and traditions, but even the most ardent critic must grant that they contain more apparent historicity and chronology than Australian concepts of 'the dream time'. In this generation, therefore, carbon 14 is likely to provide the basic clues for the interpretation of the past. This brief article is intended to record a series of dates which, it would seem, neither physicist nor archaeologist can impugn.

Despite the pressing need for a carbon 14 chronology, few age estimations are available for aboriginal cultural material. However, some of them are subject to the qualifications stressed by previous contributors to this journal. The sites have been dated by estimations on *single* samples, collected under circumstances where controlled, stratified excavation was not possible. The artifacts and the source of the samples were first exposed by natural erosion. Unfortunately, in some instances, the artifacts are such generalized types that they are of doubtful utility for cultural analysis. In the writer's opinion, the three oldest Australian stone assemblages, and the only Tasmanian midden deposit dated so far, all suffer from one or other of these limitations, and they are relevant to any assessment made of the evidence. The relevant sites and antiquity are as follows.[1] (*Dates B.C.*)

R 55/4—Cape Martin, South Australia (charcoal) . .	6740 ± 120
R 55/3—Lake Menindee, N.S.W. (mussel shell) . .	4610 ± 100
L 271E—Tartanga, South Australia (mussel shell) .	4070 ± 120
Isotypes Inc.—South Arm, nr. Hobart, Tas. (charcoal) .	120 ± 100

The credentials of two other sites, for which single dates are available, are strengthened by the observed evidence of close stratigraphy. An analysis of mussel shells retained, in 1929, by the excavators of the Devon Downs rock shelter, established their date as 2330

[1] The first three dates are discussed by Tinsdale, N. B., 1957. 'Culture Succession in South Eastern Australia', *Records South Australian Museum*, XIII, 34. Tasmanian information by courtesy of W. Bryden, Director, The Tasmanian Museum, Hobart.

± 140 years (L271E). This horizon was covered by 12 ft. of subsequent occupation, and contained uniface *pirri* points, one of the most characteristic Australian prehistoric artifacts. During 1960, the writer excavated to a depth of 8 ft. a small rock-overhang at Glen Aire, Cape Otway, Victoria. Stone material was restricted to simple flakes, but 58 bone points were recovered. A similar bone industry is distributed on eroded surfaces along the Victorian coast, and an age estimation of 538 ± 200 years (i.e. A.D. 1422) (C601) was obtained for charcoal associated with this industry in a midden near Warrnambool, 100 miles to the west.[2] At Glen Aire, charcoal from a depth of almost 6 ft., the lowest stratum to contain bone artifacts, was 370 ± 45 years old (R728) (i.e. A.D. 1590).

Thanks to the assistance of Professor Hallam L. Movius, jnr., and Dr T. A. Rafter, the writer has obtained an important series of five dates for the one stratified deposit. The gratifying consistency of the results merits comment, as the samples were collected during two seasons of excavation, in 1956 and 1958, and were dated by two laboratories.

The site, Fromm's Landing, is a large rock shelter on the Lower Murray River, 10 miles downstream from Devon Downs and 70 miles east of Adelaide. The trench penetrated almost 16 ft. of stratified occupation. Samples collected during 1958 were submitted to the New Zealand radio-carbon laboratory. The significance of these age determinations has been discussed elsewhere.[3] Age determinations of the 1956 samples were announced by the Philadelphia University Museum laboratory, during September 1960. Those samples collected in 1956 came from inside the shelter, while the 1958 excavation was concentrated in the open. However, the greatest horizontal distance between the samples was no more than 15 ft.; the vertical stratification is included in the tabulated results below.

Year Collected	*Layer No.*	*Approx. Depth from Surface*	*Sample*	*Date B.C.*	*Laboratory No.*
1958	4	6′	Charcoal	1280 ± 80	R 456/2
1956	6	8′	Ash	1796 ± 85	P 308
1956	8	10′	Charcoal and ash	1921 ± 85	P 309
1956	9	11′	Charcoal and ash	2095 ± 85	P 311
1958	10	12′ 6″	Mussel shell	2890 ± 100	R 456/1

This detailed chronology fixes the period during which pirries and geometric microliths were introduced on this site, and it also dates their disappearance from the record. The Devon Downs date for the pirrian occupation receives confirmation at this site, where pirris were current between 2890 B.C. and 1790 B.C. The adze, a basic item in the 19th century aboriginal tool kit, first appeared on this site in layer nine times. The bone gorge (or *muduk*) was employed, possibly as a fishing device, during the period of layer 7. It is salutory to reflect that while 370 years may have sufficed for the deposition of the compact stratified floor in the Glen Aire shelter, it took nine times that period to accumulate a similar thickness at Fromm's Landing.

[2] Gill, Edmund D., 1955. 'Aboriginal Midden Sites in Western Victoria Dated by Radiocarbon Analysis', *Mankind*, **5**, 51–55.

[3] Mulvaney, D. J., 1959. 'Dating of Australian Prehistory', *Nature*, **184**, 918; 1960, 'Archaeological Excavations at Fromm's Landing', *Proc. Roy. Soc. Victoria*, **72**, 53–85.

The full significance of these dates cannot be assessed until numerous other stratified series are available from all parts of the continent. The cultural sequence can then be viewed in perspective. The immediate need is for field survey, excavation and age determination in series. Recent fieldwork by the writer in Queensland, and reports of the potentialities in other states, establishes that stratified sites are available in reasonable abundance for the seeking. It is, in fact, rather easier to find your site than to obtain the services of a laboratory to date it.

Book Chronicle—*continued from p.* 36.

ISCA. *By* GEORGE C. BOON. *pp.* 38, *colour frontispiece,* 11 *plates, and pull-out plan.* A guide to the Roman Legionary fortress at Caerleon, Monmouthshire, Cardiff, the National Museum of Wales, 1960. 7*s.* 6*d.*

MANKIND IN THE MAKING. *By* WILLIAM HOWELLS. *pp.* 382, 8 *plates, many unnumbered maps and figures. London, Secker and Warburg,* 1960. 35*s.* Professor Howell's *Mankind so Far* was published in 1945 and its revised editions discontinued at his request in 1956. It was a splendid book and the first recommendation to any student or general reader. Howells now replaces his earlier work with the present one, which he describes, with a ghost of the Hooton humour that has so delighted all of us for so long, as 'a new book by a revised author'. Hardly worth reviewing because an essential for everyone: Dr Oakley writes in the publisher's blurb 'excitingly written . . . full of useful and up-to-date information. The need for such a book has long been felt in Britain'.

TOURNAI ROMAIN. *By* MARCEL AMAND and IRENE EYKENS-DIERICKX. *pp.* 169, 16 *plates,* 2 *maps,* 14 *text figures. Bruges, De Tempel,* 1960. 275 *Belgian francs.* Volume V in the series *Dissertationes Archaeologicae Gandenses* edited by Professor De Laet.

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The Late Bronze Age Chronology of Central Europe: Some Reflections

by J. D. COWEN

Mr Cowen was one of the members of an archaeological delegation, sponsored by the British Academy, which visited Hungary, Rumania and Bulgaria in April–May 1960 (*See Stuart Piggott, 'Neolithic and Bronze Age in East Europe'*, ANTIQUITY, 1960, 285). *He here writes of some aspects of Late Bronze Age chronology in Central Europe with special reference to Müller-Karpe's book reviewed by Dr Audrey Ozanne recently* (ANTIQUITY, 1960, 306).

AT a classic meeting of the Prehistoric Society, held during April 1948 in the old home of the Institute of Archaeology of London University, V. G. Childe and C. F. C. Hawkes presented two papers, each complementary to the other, on the chronology of the Late Bronze Age in Europe.[1] This joint approach was focussed on the relative and absolute dating of the Urnfields of the North Alpine foreland, whence Hawkes applied the results to the West, and pre-eminently to Britain; but, by arrangement, Childe approached the crucial area from Hither Asia by way of the Danube corridor, and from Greece through the Balkans, while Hawkes worked his way forward from the Aegean up the length of Italy, and so northwards over the Alps.

It is worth recollecting that this was the first time that British archaeologists had formally addressed themselves to this most difficult set of problems in detail, and on Central European territory. Furthermore both papers suffered, one feels, from being among the first essays in archaeological synthesis to be attempted in this country after the war, with all that is implied in that. At all events within a year of publication Childe was saying that he no longer had any confidence in his derivation of the flange-hilted bronze sword from Hither Asia[2]; and he had also by then become less assured of some of his other Asiatic derivations. While in the course of the international discussion which followed, Hawkes felt obliged to revise a number of his dates, chiefly in the direction of a longer chronology. Nevertheless, both essays commanded a splendid range of material, and were fortified by a sufficient exposition of the history of research in this field to give a sound sense of perspective. If today they have been outmoded in many particulars by the work of a small army of scholars working in half a dozen countries, they remain for English-speaking students an indispensable starting-point, and an admirable introduction to their subject.

The two papers were published in the course of 1949, and set up an immediate reaction on the continent. Though much else was involved the crux was the date of the first appearance of a fully developed Urnfield Culture in central Europe.[3] For this event Childe and

[1] *PPS*, XIV (1948), 177–95; and 196–218.

[2] His argument as published was attacked, and in my opinion successfully demolished, some years later by H. W. Catling: *PPS*, XXII (1956), 102–25.

[3] Reinecke's Hallstatt A.

Hawkes had arrived at a date of about 1050 B.C., but Central Europeans were accustomed to think in terms of a longer chronology. They considered 1300 B.C. by no means out of the question, and 1200 B.C. to be the latest possible date on any view. A difference of this order of magnitude at so comparatively recent a period was clearly intolerable. The protagonist of the long chronology was the redoubtable veteran G. von Merhart, who combined a life-long knowledge of the terrain with an undeniable bias towards the highest possible datings consonant with the evidence. For the position developed by Childe and Hawkes it was the latter who shouldered the main burden of argument. After various informal discussions issue was publicly joined in May 1950 at the First International Congress of the Prehistory and Protohistory of the Mediterranean, held at Florence, where both sides of the case were vigorously presented. The debate was continued in a friendly, not to say cheerful, spirit at the Third International Congress of Prehistoric and Protohistoric Sciences, held at Zürich in August of the same year, where Von Merhart (under the chairmanship of Hawkes as President of the Section) delivered a memorable address far exceeding the permitted limits of time. Neither did Childe, for his part, escape unscathed. In a strongly worded paper,[4] which was published on the occasion of the Congress, and in the hands of every member before the opening session, E. Vogt criticized both his attempt to change the accepted terminology, and the cavalier nature of his proposals for the handling of the cultural content of Hallstatt A and B—on both counts, in my opinion, with complete justification. In the event the known divergence of views, apparent from the first morning, made of the Bronze Age Section the most popular (with the Palaeolithic), and possibly the most brilliant, of any at the Zürich Congress.

As these lines are written it is ten years, almost to a day, since the Zürich Congress was held, and one may well ask what has happened meantime, and where we stand today. To these broad questions no detailed answer, no firm conclusions, will here be offered. But the recent appearance of a magnificent critical survey of a large part of the material,[5] coupled with the anniversary, present an occasion for taking stock, in however summary a form.

In the chronological aspect, which alone concerns us here, the outstanding development of the past decade has surely been the determined, and finally successful, effort sustained in southern Germany and Switzerland to achieve a more refined classification by subdivision of the main Urnfield periods Hallstatt A and B. In Switzerland two successive phases of Hallstatt B have been proved by vertical stratification in a dwelling-site; while analysis of the growth of certain large cemeteries in Bavaria has yielded a horizontal stratification indicating no less than three such phases. By similar methods Hallstatt A has been credited with two constituent phases, so that the whole Urnfield sequence is now organized to run in six phases: Bronze D, Hallstatt A1 and A2, and Hallstatt B1, B2, and B3.[6]

In general terms the chief merit of all this is to re-emphasize the significance of the vast mass and weight of material that is the total content of Hallstatt A and B, and to underline the implication, always present but too often overlooked, that these two periods must have taken several centuries to run their course, and cannot be crowded into a couple of brief sections of the time-span. By the same token, if such comparative longevity be recognized

[4] *JSGU*, 40 (1949–50), 209–31.

[5] H. Müller-Karpe, *Beiträge zur Chronologie der Urnenfelderzeit nördlich und südlich der Alpen*, 2 vols., 1959. Review by Audrey Ozanne, ANTIQUITY (1960), 306.

[6] With Mühlau grave 1 falling in the new Ha A1 (*loc. cit.*, 136), one is left wondering how far Müller-Karpe's Br D and Ha A1 may not after all be a revival in a new dress of Childe's D1 and D2 of the 1948 paper (*supra*, p. 40, note 1). To the extent that that is the case Ha A2 would equal old Ha A, coinciding (as in Müller-Karpe) with the appearance of leaf-shaped swords. But discussion of a point like this lies outside the scope of the present article.

and accepted then a long chronology at once imposes itself; and to that extent at least it must be said that as we stand today the odds have obviously shifted in favour of Von Merhart's general position.

In detail much remains open to debate. The nature of the relations between the newly determined phases; the extent to which the changing fashions represent cultural continuity or discontinuity; the possibility of deducing historical events to account for variations in the cultural pattern—all remain to be settled by general agreement. The very reality of the subdivisions themselves is open to challenge. In addition the scheme has been worked out in full only within a comparatively small area of southern Germany. The difficulties of applying it, even with all due modification, over wider territories are very real. To take a single example—Müller-Karpe has re-examined and demonstrated afresh the reality of Bronze D as an entity culturally separate and chronologically distinct,[7] thus confirming Reinecke's original concept as valid for south-western Germany at least. But Margaret Smith appears also to have demonstrated that in Bavaria and the East Alpine area the material distributable further west between Bronze D and Hallstatt A forms a single amalgam within which no differentiation has ever been certainly established.[8]

Many more such situations remain to be ironed out. Nevertheless it is enormously to the credit of Müller-Karpe that precisely now, cutting through all outstanding uncertainties and obscurities, he has come forward with a complete scheme, both of relative and absolute chronology, covering all west central Europe up to and including the East Alpine area. How has he achieved this? In the first place, picking up in the extreme south of Italy the trail of absolute dates there to be obtained through direct contacts with the Aegean, he has built up a nexus of correlations, province by province, from Sicily to the valley of the Po, and so north across the passes into transalpine Europe. And with the cultural correlations march the dates. Secondly, with a confidence and an optimism that one can only admire, he has applied the new subdivisions achieved in Bavaria right across the North Alpine foreland from eastern France to the point where the easternmost foot-hills of the Alps touch the first beginnings of the Carpathian Ring. Furthermore one receives the impression that by a kind of backward projection the cultural entities away to the south in Italy are so marshalled as to fall with sufficient convenience into the sexpartite scheme of the north. Such an epitome is certainly exaggerated, and may be thought unfair; but it does give a not too grossly distorted picture of what appears to have happened. The consequence is that a kind of enormous dated grid is imposed upon the cultural material of no less than twelve separately considered areas extending from Sicily to the Danube, and allocates it area by area to century after century for the best part of a millennium. And as regards the six phases of the Urnfield culture in south Germany, what do we find? Why, that each falls neatly within the limits of a single separate century from the 13th to the 8th. It is just too good to be true. It is startlingly, even scandalously, mnemonic.

Of course it stands to reason that such a scheme cannot be *absolutely* true. It is too linear, as well as too rectilinear, to have any chance of representing in detail the underlying facts of the concurrently changing situations. It is, indeed, a real fault that the possibilities of cultural overlap and of retardation here find no recognition; the result creates an impression which is altogether too Montelian for this day and age. But the apparent naïveté of the end-product must on no account be allowed to obscure the merits of the underlying argument, so fully documented in the text and plates. Müller-Karpe himself makes no claim to have written the last word; his title alone proves that. Nor does he at all suppose that every one of

[7] *Op. cit., passim.*
[8] *Zephyrus*, VIII–2 (1957), 195–240.

These two drawings were made by the Danish artist I. Kornerup in August 1861 and record the excavation by King Frederik VII and Worsaae of the great barrow ascribed to King Gorm at Jelling. The original captions to the drawings are as follows: (*above*) Kong Frederik den 7de lader bore i Gorm den Gamles Hoi i Gellinge, den 7de August, 1861 (*King Frederik VII supervising the boring into Old Gorm's Barrow at Jelling, 11 August, 1861*), *and* (*left*) Ha! ved Asathor! hvad er det for Larm i Hulen (*Ha! by Asathor! What is this awful noise in my den?*) *The excavations, it should be noted, did not find any grave or disturb King Gorm.*

See p. 2]

(*a*)

(*b*)

(*c*)

TRIPOLITANIAN DAMS

(a) Retaining wall of spillway section, Wadi Sliman.
(b) Slot for control gate in spillway, Wadi Tareglat.
(c) Soil-retention dam in Wadi Ganima. The silt accumulated behind it (right) is now being eroded.

See pp. 14–20]

(*a*)

(*b*)

TRIPOLITANIAN DAMS

(*a*) *Ruined masonry dam in tributary of the Tareglat; fortified farm* (*gasr*) *in background.*
(*b*) *Dam reinforced by buttressing and stepping* (*Tareglat basin*).

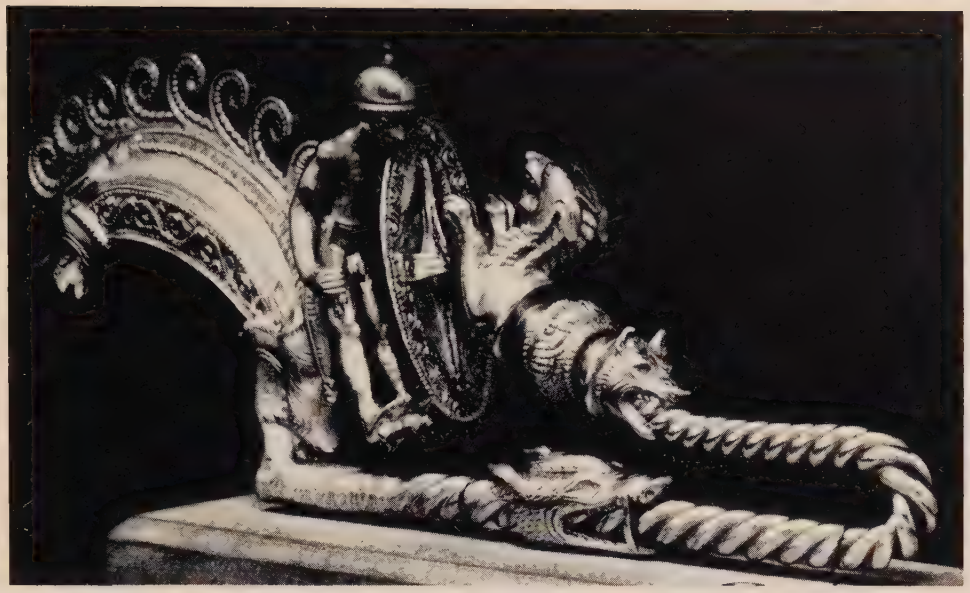
See pp. 14–20]

(*a*)

(*b*)

TRIPOLITANIAN DAMS

(*a*) *Reservoir dam in lower Caam, with sloping spillway on the left.*
(*b*) *Lower Tareglat: a part of the curved soil dam.*



A CELTIC WARRIOR

A golden statuette of a Celtic warrior, soldered to a brooch of fine workmanship. The statuette is approximately 1¾ in. high.

See p. 53]

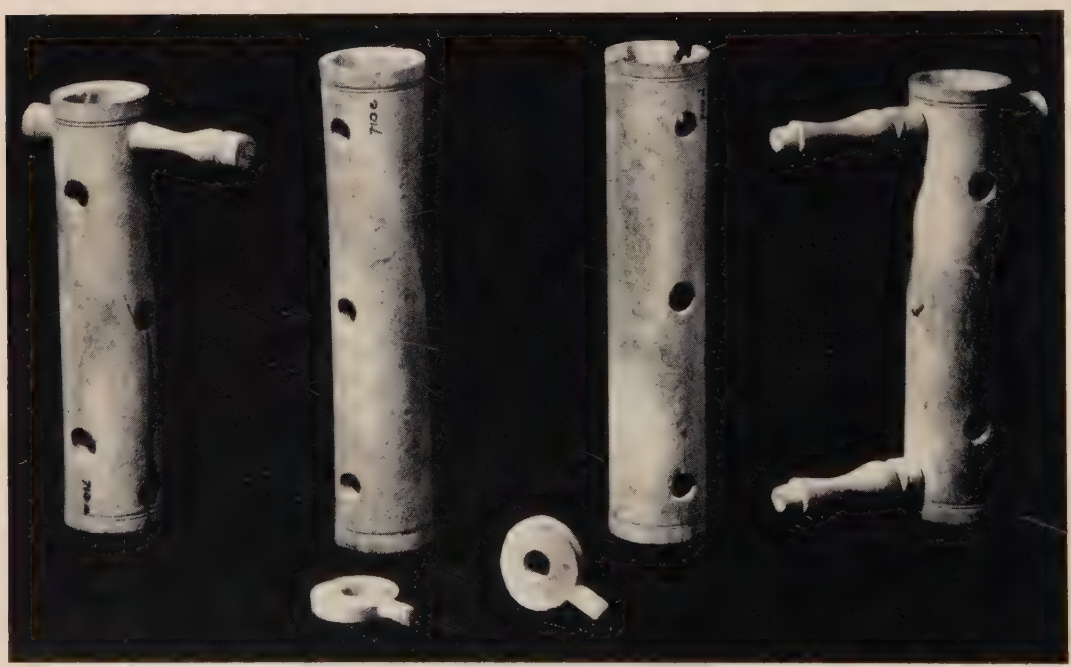
(*a*)

MYCENAE

(*a*) *One of the eight Linear B tablets found in the citadel* (*length of the whole* $4\frac{1}{2}$ *in.*).

See p. 57]

(*b*)



PENNY WHISTLES

(*b*) *St Servaas, Maastricht: bone objects from the late Roman cemetery.* (Limburgs Provinciaal Museum voor Kunst en Oudheden). *Scale* $\times \frac{2}{3}$.

(*Photo: F. Lahaye*).

See pp. 55–7]

PENNY WHISTLES

Mesará Plain, Crete: shepherd playing on a bone pipe.
(Photo: Ronald Doyle, Newquay).

See pp. 55–7

(a)

(b)

PRESERVATION—IN A GLASS CASE

(a) The old church at Zsámbék, Hungary. *(b) A study model of Professor Pogany's plan to case in with glass the ruins of the old church at Zsámbék.*

See p. 58]

his six phases did last just one hundred years, yielding each to the next precisely at the turn of a century. What he has done is to find, with varying degrees of assurance, a date of some kind for each of his phases; and to note that these dates are in fact distributed over each of six successive centuries.[9] This means that there is still room for anyone to more than double the length of some of the phases, and to more than halve others, without the necessity of moving a single one of Müller-Karpe's dates. It also follows that if only a majority of his dates are approximately correct, and it looks as if they well may be, then the general picture he presents cannot be far wrong. His achievement is to have argued afresh and *ab initio*, using new material from his own researches in Italy, the bases of the chronology of the Urnfield culture in the North Alpine foreland, so far as this can be made to rest on Italian factors; and to have presented a framework of absolute dates, strongly if roughly articulated, which yet gives scope for considerable flexibility, as future research may show to be desirable, without impairing the validity of the structure as a whole. It is already safe to say that his book has closed a long and confused chapter in the history of the subject, and will form a monumental, though not unchallenged, basis for all future progress.

It will not have escaped notice that in this new study Müller-Karpe has used the same line of approach as did Hawkes twelve years ago. Have we then any up-to-date counterpart to Childe's companion study of 1948? Alas, we have not. It is indeed doubtful if the present state of research in the Danubian countries would yet support anything more substantial than a skeleton outline of the kind Childe has already given us, much less a close survey such as Müller-Karpe's. In Hungary the long-drawn-out attempt to build a chronology covering the whole Bronze Age on the basis of the material derived from the great tells has at last been given up, and that is something gained; but apart from the Early Bronze Age, which has never lacked attention, leading scholars there have recently concentrated on the construction of an independent sequence of bronze types for the Middle Bronze Age,[10] and have not yet come to grips with a detailed consideration of the types and cultures contemporary with the West Alpine Urnfields.[11] Research in Yugoslavia seems to have made little

[9] More precisely defined his method is that broadly imposed by the circumstances on all students of the subject; that is, his first and last periods—Br D and Ha B3—are tied in by a series of synchronisms to the 13th and 8th centuries respectively, the first being dependent on Late Mycenaean dates ultimately resting on the written history of Egypt, and the last on South Italian and Sicilian dates ultimately resting on the written history of Greece. Working inwards from these two extremes the periods next after Br D and last before Ha B3—i.e. Ha A1 and Ha B2—are with some show of probability assigned respectively to the 12th and 9th centuries. Thereafter two centuries remain in the centre to accommodate the periods Ha A2 and Ha B1, which are accordingly assigned to the 11th and 10th centuries. The whole process is in fact indistinguishable from Hawkes's 'stretching the elastic across a nasty gap' (*PPS*, XIV (1948), 205, 207); both authors, oddly enough, found the elastic just nicely sufficient to do the job, but with very different results for the absolute chronology of the North Alpine Urnfields.

[10] The outstanding study here is that of Dr A. Mozsolics, *Acta Arch. Hung.*, **8** (1958), 119–56; supplemented for finds of gold by the same author, *ibid.*, **9** (1958), 253–63. A revised view by I. Bóna, *ibid.*, **9** (1958), 211, is not so acceptable.

[11] The Late Bronze Age in Hungary is currently subdivided into B IV, V, and VI, very roughly corresponding to Reinecke's Br C/D, Ha A, and Ha B. The ceramic cultures associated with these stages are those of Egyek/Piliny, Vál I and Vál II respectively, while in Slovakia the Piliny, Velatice, and Podolí groups seem to cover the equivalent time-span. The study by A. Mozsolics just quoted (*supra*, footnote [10]) deals fully with bronze hoards of the transition B III/IV. A recent review by the same author of openwork bronze cart- or chariot-fittings covers the periods B IV and V, but is strictly limited in scope: *Acta Arch. Hung.*, **7** (1958), 1–14.

Cremation cemeteries with a ceramic typical of the Vál culture are steadily coming to light in northern Pannonia, either from new excavations or from re-examination of old material; but the distinction between I and II (Ha A and B) is often blurred. Cemetery of Tőkől, E. Patek, *Budapest Regiségei*, XVIII (1958), 385–424; Neszmély, publication forthcoming; Mezőczát (to the north-east in Co. Borsod), ultimate Ha B, with large iron beads and other ornaments—the first appearance of iron in Hungary. [*note continued p.* 44.

or no progress with the construction of an independent bronze age chronology, and in the north and centre is forced to rely on extrapolations of Reinecke's scheme, employing such of the new subdivisions as may seem appropriate.[12] This affords a certain convenience to western workers; but the position will not be really satisfactory till a joint scheme can be worked out in conjunction with Hungarian colleagues. In Rumania much work has been done towards the systematic collection of material for research, but as yet not much by way of synthesis, at least in the Late Bronze Age. For Transylvania a provisional independent classification of the relevant hoards is being worked out, which takes account of the latest developments of research in southern Germany; but approximate equivalents of all the phases have not yet been recognized even in this rich material.[13] It is by no means certain that all are present, and there is not the slightest reason why they should be. In any event the provisional character of the classification is emphasized by local workers, and it is still subject to repeated modification.

Transylvania has long been renowned for the wealth of its prehistoric cultures. That is specially true of all periods of the Bronze Age; the number, size, and weight of the bronze hoards in the museum at Cluj have to be seen to be comprehended. South of the Carpathians the situation changes abruptly; in the plain of Wallachia bronzes are already scarce, over the Danube in Bulgaria they are rarities. Here indeed we are face to face with that archaeological gap extending right across the Balkan peninsula from Albania to the Black Sea, and separating northern Greece from the Danubian lands, which was formally deplored by the First Congress of Prehistoric and Protohistoric Sciences meeting in London in 1932. Since then something, no doubt, has been done by excavation and research to narrow the gap for the Neolithic Period; but in Bronze Age studies it yawns as wide as ever.

Such are some of the difficulties in the way of resuming and furthering Childe's work towards establishing a chronology for the Alpine Urnfields from Mycenaean and Iron Age Greece by way of the Balkans and the Danube. Some we may expect shortly to see removed; others will be more intractable. But until Childe's pioneer work can be renewed with some reasonable chance of achieving assured results, we shall lack one characteristic and essential element of the well-planned attack launched by Childe and Hawkes in 1948—an alternative chronology, independently achieved by a separate route, and available once and for all to settle the dating of the Alpine Urnfields by the confirmatory clinch of a pincer movement.

[1] *note continued from p. 43.*]

This note and the two next are intended as a short supplement to the account of recent work in Eastern Europe given by Professor S. Piggott in ANTIQUITY (1960), 285–94, bringing the story down to the end of the Late Bronze Age. The material available does not, unhappily, suffice to support a general synthesis such as he has presented from the Neolithic to the close of the Middle Bronze Age.

[12] Slovenia: S. Gabrovec, *Najstarejša Zgodovina Dolenjske* (The earliest history of Lower Carniola), Novo Mesto, 1956. Croatia: Z. and Ks. Vinski, *Opuscula Archaeologica*, Zagreb, I (1956), 57–109 (bronze hoards). Serbia: D. Garašanin, *Katalog der vorgeschichtlichen Metalle*, Nat. Mus. Beograd, 1954.

[13] I. Nestor, *Marburger Studien*, 1938, 192; M. Petrescu-Dîmboviţa, *Anuar. Inst. Stud. Clasice*, Cluj, V (1944/48), 273 and notes, 280–81; cf. *idem*, *Dacia*, N.S. II (1958), 63. Key hoards: Uriul (Felőr), M. Roska, *Thesaurus*, Cluj, 1942, 86–7, figs. 106–7; Domaneşti (Domahida), J. Hampel, *Bronzkor* I, 1886, pls. CXXII–CXXIV; Tauteu, V. Dumitrescu, *Dacia*, V–VI (1935/36), 225–34, figs. 1–7; Guruslău, M. Moga, *Dacia*, XI–XII (1948), 257–64, figs; Fizeşul Gherlei (Őrdőngősfűzes), Hampel, *Arch. Ért.*, XV (1895), 196–201, figs.; Sîngeorgiu de Pădure (Erdőszentgyőrgy), A. Mozsolics, *Közlemények*, Cluj, I (1941), 100–108, figs., and Roska, *Thesaurus*, 75, fig. 90.

Across the North Sea: A Review

by J. V. S. MEGAW

Prompted by last year's Netherlands conference of the Prehistoric Society, Mr Megaw notes some of the significant points for the wider horizons of British and European prehistory contained in three recent publications on the archaeology of the Low Countries.

THE LOW COUNTRIES (*Vol. 5 in the series 'Ancient Peoples and Places'*) *by S. J. De Laet. London: Thames and Hudson,* 1958. *pp.* 240, 63 *plates,* 34 *figures* (*including one table and a map*). 25*s.*

DE VOORGESCHIEDENIS DER LAGE LANDEN by S. J. De Laet and W. Glasbergen. Groningen: J. B. Wolters, 1959. *pp. xvi* + 221, *colour frontispiece,* 48 *plates,* 72 *figures and a table. f* 16.90.

HONDERD EEUWEN NEDERLAND (Antiquity and Survival, *Vol. II, Nos.* 5–6) *edited by J. E. Bogaers, W. Glasbergen, P. Glazema, and H. T. Waterbolk, 2nd impression,* 1959. *pp.* 314, *ill.* 337 *and a table in two parts. f* 17.30 (*paper covers*), *f* 19.80 (*cloth*).

EXACTLY three centuries ago last year, Johan Picardt, parson of Coevorder in Drenthe and Adviser for Land-Reclamation, wrote in a regional study worthy of comparison with his contemporary John Aubrey's *Monumenta Britannica:* 'Our fore-fathers had not pen, ink, nor paper, which had they possessed would have enabled us to learn of strange things. Howsomever, they have bequeathed us signs enough if only it were that we could read them.' Each one of the three volumes from the second of which my quotation comes clearly shows the ways in which archaeological research in the Low Countries has vindicated Picardt's statement. Today archaeology in the Netherlands is a matter of exemplary close-knit co-operation between State institutions, universities, local societies, and the private individual, while recently Belgium has taken important steps in the systemization of pre- and protohistoric studies.[1]

Although the Low Countries can hardly be regarded as a primary area of development, their coastal position at the north-western limit of the transcontinental routes has made them a cultural entrepôt of particular significance for the British archaeologist. In this context Professor De Laet's book in the 'Ancient Peoples and Places' series is all the more welcome in that it is the first English summary of the pre-Roman archaeology of the region.

De Voorgeschiedenis der Lage Landen is a much expanded and corrected version of De Laet's volume, with Professor Glasbergen's collaboration giving a more balanced account of the Dutch evidence. The third book, a double volume of *Antiquity and Survival*, offers a lavishly illustrated symposium of one hundred centuries of the Netherlands, and although the promised English language edition is not to be, the Trustees of the Prince Bernhard Foundation and the Rijksdienst voor het Oudheidkundig Bodemonderzoek are to be warmly thanked for giving their support to *Honderd Eeuwen Nederland.*

In examining some of the significant and often unfamiliar facts which arise from a study

[1] ANTIQUITY (1959), 291–2. For Picardt: K. H. Jacob-Friesen in *Niedersachsens Urg.*, 23 (1954), 3–19.

of all three books, the story starts with the rolled implements of Clactonian form from the Spiennes gravels, the earliest identifiable industry in the Low Countries. However, not until the expansion of the North German Hamburgian reindeer hunters is human settlement recorded in the Netherlands north of the great rivers. Dated to the Allerød oscillation and Younger Dryas, the Tjonger culture of N. Belgium and S. Holland, as Glasbergen notes, has many points of resemblance to our own Creswellian. Professor Waterbolk in his *Antiquity and Survival* article (which includes an admirably clear explanation of radiocarbon dating[2] neatly balanced by Dr van Zeist's article on palaeobotany in the same volume) dates the Ahrensburgian site of Geldrop near Eindhoven 8765 ± 85 B.C. (Younger Dryas). A second important date is that for the earliest attested boat in Northern Europe, the 10 ft. long pine-wood dug-out from Pesse (Drenthe); at 6315 ± 275 B.C. it agrees satisfactorily with its botanical dating in the Boreal. The Tardenoisian of the south of the Low Countries is established in the Boreal/Atlantic transition and a notable discovery are the huts of wattle-and-daub recorded at La Roche aux Faucons. The heavy flint industry which typifies the so-called 'Campignian' regarded by Dr Irwin Scollar simply as factory wasters, is considered to belong to a residual Mesolithic tradition influenced in part from the Scandinavian Ertebølle.

Danubian farmers spreading westwards along the loess of the Maas and Jeker to the edge of the Atlantic mixed oak-forests represent the earliest true Neolithic settlement. The rapid rate of expansion from the parent regions is indicated by the similarity with the classic sites such as Köln-Lindenthal or Bylany. Sittard (described in *Honderd Eeuwen Nederland* by its excavator Dr Modderman) well bears this out as do the dates of 4175 ± 60 B.C. for Geleen and 4015 ± 85 B.C. for a Danubian I sample from Vinča 'A'.[3] Stone was imported from the Rhineland for the manufacture of shoe-last celts; exotic pottery types such as the Hinkelstein ware from Stein and Sittard also occur, but the short duration of this Danubian settlement is underlined by the manner in which the 'pre-Campignian' elements, temporarily submerged, came once more to the fore in the expansion of the later Neolithic period and it is here that the *tranchet* picks of the Spiennes mines take their place; De Laet and Glasbergen see these as clearly of pre-Campignian descent; on the vexed problem of the Michelsberg culture which extended as far as Dutch Limburg, De Laet and Glasbergen stress the typological links with Windmill Hill from antler combs to long barrows—though the Krakelbos, Ottenburg, mound is perhaps doubtful evidence here. In *Der Lage Landen* the question of origin is only cautiously touched on but in general the bias is towards the 'western' late Chassey view as recently expounded by Scollar as opposed to the Vogt-Becker-Piggott theory.[4]

A second point at issue in the later Neolithic is the evolution of the characteristic collective *hunebedden* concentrated on the Drenthe moraines. In *Honderd Eeuwen* Mrs Lili Kaelas summarizes her recent findings. It is now clear that the *hunebedden* with their structural similarity to our own Medway tombs, do not represent an ancestral form to the First Northern flat graves and cist burials but run parallel with them, the former belong to a separate and pre-megalithic tradition, the latter date from TRB 'C'; the collective Passage Grave element as in the Bronneger triple burial, is correspondingly later. Mrs Kaelas claims the Netherlands as the area where the TRB culture first felt the influence of the western megalithic tradition, though compared with the Danish material, the absence of 'fruitstands' and pottery ladles from the Dutch tombs is noticeable; in contrast oculi

[2] To the best of my knowledge none of the Groningen dates here quoted has been corrected for the 'Seuss effect'; *q.v.* H. T. Waterbolk in ANTIQUITY (1960), 14.

[3] Waterbolk, *op. cit.*, 15.

[4] *PPS*, XXV (1959), 110–6.

decorated bowls are found with the early 'Drouwen' phase flat based pots. A decline in elm pollen marks the inroads of a gradually expanding population; a C14 date of 1896 ± 150 B.C. for D 52, Diever, must be late, contemporary with the Nieuw-Dordrecht trackway (1885 ± 55 B.C.) and its solid single piece oak wheel, now to be paralleled in an example from Eese.[5] Jet and amber beads as well as copper spirals of Jordanów form illustrate the far-reaching contacts of the Dutch TRB settlers. Of a number of coastal 'secondary' Neolithic settlement sites contemporary in their early stages with TRB 'B', the spectacular finds at Vlaardingen (S. Holland) are too recent for inclusion in any of the three volumes under review; the lower levels of Zandwerven (N. Holland) contain flared mouthed vessels with pointed bases of distinctly Ertebølle appearance. Above are the westernmost examples of the early protruding foot beakers.

Of the S. Belgian SOM settlement only a few monuments have survived, notably the two well-known Gallery Graves at Wéris (Lux.), both having porthole entrances to the burial chambers.[6] Widespread trade can be noted in finds of jadeite axes with an especially fine and truly Breton-looking example coming from Havenne (Namur), Grand Pressigny daggers—which also occur in Dutch beaker contexts[7]—and amber beads. De Laet considers that the famous miniature 'Stonehenge' of Forrières originally consisting of six trilithons may belong here, but there is nothing in the absence of firm dating evidence to preclude a Bronze Age date.

Turning to the Beaker period proper, Glasbergen and van der Waals summarize their original theories of the dual nature of the colonization[8] in *Honderd Eeuwen Nederland*. The earlier group is that represented by the corded-ware influenced protruding foot beaker, decoration being restricted to the area between the rim and the point of maximum circumference. Associated finds include long unretouched flint knives, flint axes and a few of Glob's 'pan-European' battle-axes. One of these was found with a flint knife and protruding foot beaker at Ede, Gelderland (2240 ± 120 B.C.). Several centuries after the first appearance of people using protruding foot beakers, who seem to have early lost their urge to expand, is the eastern incursion of the first true bell-beakers of 'Maritime' type. A local evolution is the contracting of the alternative decorated and plain bands into three zones, the type of the famous Odoorn barrow group—not a pan-European beaker as De Laet terms it in the *Low Countries*. The Odoorn tanged knife has been shown by Dr Junghans to be of Alpine copper, while the awl is associated analytically to material from the south-western German Early Bronze Age. The two gold sheet ornaments may on the other hand reflect a Breton origin. In addition to scattered finds of amber V-perforated buttons and two- and four-holed wrist-guards, the presence of a typical beaker assemblage is emphasized by the discovery of a Bohemian beaker in a barrow at Harskamp (Gelderland), a form which must have influenced the local floruit of the richly decorated 'Veluwe' variant with its characteristic metopic designs. It is, however, the contracted zone beakers which represent the major period of expansion; long realized as a vital point in the ancestry of our own northern short-necked 'C' beakers[9] later to evolve into the native long-necked 'A' form. The

[5] Information from Professor Waterbolk; the pollen dating of this specimen fits this context. Two other undated wheels of this type are in the Provinciaal Museum van Drenthe, Assen.

[6] T. G. E. Powell has commented briefly on the significance of the free standing menhir; ANTIQUITY (1960), 190.

[7] N. K. Sandars, *Bronze Age Cultures in France* (1957), 38 summarizes Mariën's separate SOM and Beaker colonies south and north of the Demmer/Dyle line with their common flint trade which Sieveking's recent work indicates covered a short period in the 17th century B.C.

[8] *Palaeohistoria*, IV (1955), 5–46.

[9] M. E. Crichton Mitchell in *PSAS*, LXVIII (1933–4), 153–8.

all-over corded type which with an overflow from the German Rhineland area resulted from a blending of corded- and bell-beaker elements is, like the pot-beaker and barbed-wire decoration, a variant[10] which also occurs on the East coast of Britain. Indeed a typical British necked beaker is recorded from a barrow on Elspeet Heath in the Veluwe. This is the context for the occasional finds of Grand Pressigny daggers which are often associated with the zig-zag decorated beakers in which Glasbergen and van der Waals see TRB influence; one such with a drooping blade axe found at Eext (Drenthe) is dated to 1685 ± 50 B.C.

It is unfortunate that the associated funerary structures of beakers in the Netherlands are so much less susceptible of classification than the beakers themselves. Small diameter post circles closely set in a bedding trench enclose Odoorn and other protruding foot beaker crouchers, but in Glasbergen's opinion there is no clear example of a single widely spaced timber circle associated with a beaker grave.[11] However it would be interesting to know more of the status of British close-spaced post beaker barrows. The Netherlands contains only two or three of the so-called 'beehive' tombs, Holwerda's *Kuppelgräber*, despite the prominence given them in the English volume, and are associated once more with protruding foot beakers. The early part of the true Bronze Age, dated here 1500–1300 B.C., is perhaps a little late for the inclusion of such stone-work indicative of renewed Scandinavian contacts as late battle-axes and fine flint daggers. The flint sickles which both De Laet and Glasbergen would place here in fact in no case occur in contexts which can be dated before the Iron Age. The Heiloo (N. Holland) bronze example found with four flint sickles is certainly of no known Bronze Age form. Despite the inconsistencies of De Laet's Montelian chronology on pp. 114, 127 and 179 of *The Low Countries*, to this general 'Neolithic' level belong some dozen thick butted Alpine type copper axes and the round headed dagger from Bargeroosterveld (Drenthe). Presumably a Únětice import of the south Drenthe bell-beaker folk, as perhaps the Bennekom gold ornament, it recalls not only the Hammeldon Down (Devon) amber hilt, but also the bone pommels from Manton and elsewhere.

Although the subject divisions of both general works does not make easy reading for those unfamiliar with the overall picture, the widespread foreign contacts of the Bronze Age are well illustrated in all three volumes. In *Honderd Eeuwen Nederland* Dr Jay Butler examines these contacts on the basis of some half-dozen hoards which will also comprise the first Dutch set in *Inventaria Archaeologica*. The earliest group is that from the coastal site of Waginingen (Geld.). Although the closed nature of the find is disputed, several objects support Butler's claim for an Irish origin. A thin butted axe and two plain armlets recall the Migdale (Sutherland) hoard[12] while an ÓRíordáin type 6 halberd, one of three in the Low Countries here compared with Killaha East (Co. Kerry), is, following Ap Simon, placed in the latter's Wessex I.[13] To such evidence of Irish trade may be added the Fauvillers (Lux.) lunula and a fragment of a Megaw and Hardy type I decorated axe from Haren (N. Brabant).[14] In view of such evidence of brisk trade it is strange that no bronzes have ever been recovered from Veluwe burials or their southern contemporaries—a disturbing lack

[10] All-over corded and pot beakers: H. J. Case in *PPS*, XXV (1959), 43–4; barbed-wire decoration: I. M. Smith in *11th Ann. Report Inst. Arch.* (1955), 34–41, and for a protruding foot beaker burial and the use of such decoration on a 'Hilversum' urn: P. J. R. Modderman in *BROB*, 9 (1959), 286–9.

[11] Munro Lecture, University of Edinburgh, 4 March, 1958 (unpublished); contrast P. Ashbee, *The Bronze Age Round Barrow in Britain* (1960), 60–5.

[12] Most recent publication with full references in *Inventaria Archaeologica: Great Britain* 5th set (1958), G.B. 26.

[13] *10th Ann. Report Inst. Arch.* (1954), 40.

[14] P. J. R. Modderman and J. J. Butler in *BROB*, 9 (1959), 290–2; a second crude type I axe of doubtful provenance is in the Museum Kam, Nijmegen.

for those who would see here any Netherlands influence in the rich decoration of Class 'A' food-vessels.

In contrast to the Scandinavian and North Saxon contacts illustrated in the Northern Netherlands, in the south of the Low Countries links with the Sögel culture are shown by finds of the typical rapiers, nicked axes and palstaves. The mortuary house of the well-known Drouwen barrow (Dr.) contained not only a dagger and axe but also the paper-thin British Class I razor.[15] This burial with its 'Bagtorp' Montelius I spears and a date which places it at the beginning of the Dutch Middle Bronze Age brings us to the British merchant's hoard of Voorhout (S. Holland) which includes a trunnion celt paralleled in the Lusnach (Co. Offaly) find and 'shield' ornamented palstaves of the Acton Park (Denbighs.) type.[16]

Here we come to another point of Anglo-Dutch contact; Dunning's 1936 claim for a trade link with the Deverel-Rimbury culture evidenced by a group of sherds from Hilversum and Boulogne-sur-Mer distinguished by a decorated collar with internal bevel followed van Giffen's suggested British relationship for his secondary bucket-shaped urn from the Late Neolithic Groot-Drakenstein barrow. Now, Glasbergen's discoveries in the banked twin tumulus 1b at Toterfout-Halve Mijl—so like one of our own complex Wessex bell-barrows—contained not only four secondary Drakenstein urns but a central cremation in a Hilversum urn dated by C14 to 1495 ± 100 B.C. Isobel Smith has commented on the similarity between this vessel and those from the Tyning's Farm (Somerset) and Windmill Hill barrows.[17] The pollen spectra for tumulus 1b proved to be similar to that from the nearby monumental Zwartenberg banked barrow, Hoogeloon in which a Montelius III palstave-chisel was found in 1846. A postulated route of cremating Wessex II settlers from N. France is marked in Belgium by ten 'bell' and 'disc'-type barrows, while the twin turf-filled 'ritual pits' of tumulus 1 of the Eight Beatitudes is another truly Wessex innovation. Hilversum settlements at Wijchen near Nijmegen and Boreshoten (Gld.) contemporary with Veluwe beaker forms, in the absence of evidence for extensive agriculture must have represented mere trading posts. The position of Voorhout on a coastal sand bar is also significant. Nor can there be so clear an example of the extent of contemporary Bronze Age trade as the necklace from the Exloo, Odoorn, barrow with its 43 beads of amber, faience and tin. The Drakenstein urns, which need no longer be taken with Late Bronze Age ring ditch urnfields, have a somewhat more widespread distribution[18] and must be seen as a local development; with one exception they occur as secondaries in early barrows.[19]

This is the period of the first widely spaced timber circle barrows in the Low Countries, the Galgenberg at Sleen (Drenthe) producing a Montelius II palstave, a twisted bronze bracelet, two gold spirals and fourteen bronze arrow-heads. The evidence obtained by Glasbergen for temporary funerary structures of the type known both from Reinecke B2 graves and the Poole (Dorset) barrow I[20] together with the complexity of the presumed early tumulus 75 at Zeijen (Drenthe) with its five central inhumations, mortuary house,

[15] J. J. Butler and I. M. Smith in *12th Ann. Report Inst. Arch.* (1956), 22–33.

[16] M. A. Smith in *PPS*, XXV (1959), 165–8.

[17] *Honderd Eeuwen*, 120 has the captions reversed to figs. 28 (Brachterbeek: also comparable with Tyning's Farm) and 29 (Budel: compare barrow B. 47, Bulford). Pot 2 from Gorftad, Co. Londonderry, has a very 'Hilversum' look; A. McL. May and A. E. P. Collins in *UJA*, 22 (1959), 33–9 and fig. 2. The important recently excavated settlement at Vogelenzang, near Haarlem, shows a coastal variety closest to Mildenhall; J. G. D. Clark in *Ant. J.*, XVI (1936), 29–50.

[18] *Voorgeschiedenis der Lage Landen*, fig. 58 has the Hilversum and Drakenstein symbols reversed.

[19] Piggott's review of the 'Eight Beatitudes' paper, *Ant. J.*, XXXV (1955), 235–7, was the first realization in print of the significance of this to our own Deverel-Rimbury ancestry.

[20] H. J. Case in *PPS*, XVIII (1952), 148–59.

eight secondaries and long parallel post avenue must surely belong to a similar ritual tradition as produced Woodhenge. A swing away from trade with Britain to Central and N. Germany is shown in a *c.* 1100 hoard from Veenenburg; three decorated knobbed pins and five penannulars are of typical South German Hallstatt A urnfield type. Contemporary are the complex and double post circle barrows (e.g. those of Toterfout-Halve Mijl) and the later Drakenstein urns (not Montelius IV and Late Bronze Age as De Laet terms them).[21]

The beginning of the Late Bronze Age sees two main areas of formative influence; the Scheldt with contacts with N.W. France and Britain and the Maas/Meuse (E. France, N. Switzerland, and W. and S. Germany). One of several hoards from Bargeroosterveld (Drenthe) belongs to the beginning of the period with its two Curwen type 'C' palstaves similar to those from the Late Bronze Age I Nettleham and Wilburton finds and a pair of developed Montelius IV 'Nierenringe' swollen-ended incised-decorated bracelets of Sprockhoff's Ems-Weser group.[22] Despite finds of British palstaves, objects of M. A. Smith's 'ornament horizon' are rare in the Low Countries, an exception being the four plain ended torcs from Dave (Namur).[23] In contrast, northern influence is strong in a group from Drouwen, including a spectacle brooch and fragments of a fine girdle-box with meander pattern.

In the later Urnfield period (Late Bronze Age II/'carp's tongue complex') there are a number of interesting finds important in any consideration of our own final Bronze Age. Apart from a few square-mouthed Breton axes in Belgium, socketed axes similar to the British South-Eastern type occur in several large hoards in the south of the Low Countries, while winged axes are concentrated in the region of the Maas/Meuse, the Scheldt, and with an extension into Picardy. A bronze mould for casting a faceted axe in the Atlantic tradition from Havelte (Drenthe) has its closest parallel in the now lost Quantock Hills (Somerset) find.[24] Of two axes from the 1896 Bargeroosterveld find one is an East German import (or copy) of Seddin type while the second has the Ems *profilierte Tüllenmund* seen in the Birchington (Kent) hoard dated by Hawkes and M. A. Smith to *c.* 600 B.C.[25] A number of swords from find spots near the mouths of the great rivers belonging to this general horizon[26] include both a considerable number of *Griffzungenschwerter* and English sword types from the south[27]; basal looped spear-heads occur in the Northern Netherlands.

De Laet conceives all this material as belonging to two groups of Urnfield cultures—in *Voorgeschiedenis der Lage Landen* the uncompromisingly Lusatian origins of the English volume are rather more happily played down. North of the Rhine is a 'proto-German' region beyond the influence of the true Urnfields; the pottery as at Wedderveer and Harenermolen[28] is of biconical Westphalia-Hanover form. The key-hole ditched graves of the later cemeteries also indicate Westphalian influence.[29] Both, however, in the north and south—De Laet's 'Celtic' zone—where the associations are with the true Western group, the occurrence of palisade and ditched barrows must in part at least go back to the early

[21] Here one may point out that M. A. Smith emphasizes the Drakenstein nature of the Ramsgate urn; *op. cit.*, 161.

[22] The Bargeroosterveld hoards are fully described by J. J. Butler in *Provinciaal Museum van Drenthe: Museumbulletin* 1959 (1960), 31–57.

[23] M. A. Smith, *op. cit.*, 163.

[24] *Archaeologia*, XIV (1803), 94–5 and pl. XXVII, fig. 1.

[25] *Ant. J.*, XXXVII (1957), 185.

[26] A carp's tongue sword from Hamme (E.Fl.) should be added to Cowen's list in *PPS*, XVIII (1952), 145–6.

[27] *Ibid.*, 144.

[28] = secondary burials in a barrow, one vessel containing a Northern swan's neck terminal razor.

[29] Dr Glazema's article in *Hondsrd Eeuwen* on churches and burial customs of the medieval period has some interesting comments to offer; *q.v.* 240–3.

Bronze Age traditions. The occasional occurrence of 'Kerbschnitt' in the southern group is taken by De Laet to owe something to the Alsace-Palatinate Middle Bronze Age—an unsupported view not followed in the Dutch volume.

Within Hallstatt C we have the spread into central and highland Belgium and the southern Netherlands of a warrior group of barrow users and the Hallstatt C/Montelius VI Gündlingen form of *Griffzungenschwerter* occurs on a number of southern sites in the region of the Maas while antennae examples also occur as the iron weapon of Court-St-Etienne, tumulus 3 (not 'V' as indicated by De Laet), one of a group of *Fürstengräber*, either of warrior-traders or mercenary adventurers—that most established of Celtic professions.[30] The little known and as yet not fully published Wijchen (Gld.) harness trappings and cart fittings described and illustrated in *Voorgeschiedenis der Lage Landen* contained a pair of two-link bits, typical Hallstatt decorative open-work and two pairs of axle caps, the linch pins being decorated with twin human heads. The Oss (N. Brabant) barrow with its plank-lined grave, Italic ribbed situla, and bent iron sword with gold-studded hilt and 'peaked cap' pommel is another clear link with the central Hallstatt region. The earlier burial practices continue however—low barrows in the Kempen (e.g. Lommel-Kattenbos) and peripheral ditches with carefully aligned entrances in the Netherlands. Pottery of particular interest is the so-called 'Harpstedt' ware which Glasbergen would take to be a local derivation from the Hilversum-Drakenstein urns, and the distribution of the type—too southerly in the main for a true German point of origin as Bursch pointed out—is not incompatible. However that may be the flat-based situlate type with finger tip decoration and a roughened lower part is strikingly like Hawkes's British First Southern 'A' pottery. In the vicinity of the northernmost urnfields are Picardt's 'heidense legerplaatsen', the Dutch equivalent in every respect of our 'Celtic' fields, Steensel (N. Brabant) being dated *c.* 500 B.C. by pollen analysis.

In the final stages of the pre-Roman period of the north against a general pattern of continuity from Urnfield times is a scatter of Jastorf material and a few rich finds of later La Tène metalwork such as the La Tène II anthropomorphic dagger with lattice work handle similar to an example from Bury St. Edmunds.[31] In this area too the custom of heaping mounds over the cremation pyre continues well into the 2nd century A.D. The falling sea level *c.* 300 B.C. dates the establishment of the first 'terpen' of Friesland and Groningen. Their continuous occupation until the building of the first enclosure dykes in the 19th century is graphically illustrated by H. Halbertsma in *Honderd Eeuwen* on the Northern coastal culture. The famous Ezinge terp with its characteristic Weser-Elbe aisled long houses[32] has its first three levels clearly associated with material of Boeles's proto-Frisian period which ends *c.* 50 B.C. with the movement of the Germanic tribes. The fact that the earliest proto-Frisian settlements with their geometrical decorated pottery belong to Westergo, the westernmost province, seems to point to a seaborne colonization from Northern Germany.

True La Tène centres do of course exist in the Low Countries; the 'Marnian' influenced Hainaut region, the Hunsrück-Eifel highland zone of Belgium, and the Southern Dutch provinces. Continuity from the previous periods is clearest in the Lommel-Kattenbos barrow group. This is the region of the richest finds of the period. Eigenbilzen (Belgian

[30] A useful summary of Mariën's republication of the Court-St.-Etienne material is offered by J. M. Coles in ANTIQUITY (1960), 314–6. Add to the 'stimulus' from tumulus 3 a second socketed goad allegedly from the Rhine at Nijmegen now in Leiden.

[31] *q.v.* E. M. Jope in Problems of the Iron Age . . .: Inst. Arch. Occ. Paper No. 11 (1961), 73.

[32] Feddersen Wierde offers the most recently published example; *q.v.* W. Haarnagel in *Neue Ausgrabungen in Deutschland* (1958), 215–28.

Limburg) is a chieftain's grave by any standards with, as at Oss, the cremated bones contained in an Italic bucket in addition to a complete Etruscan beaked flagon and the fragmentary example with its open-work 'Waldalgesheim' gold ornamental band. To the end of the period belongs the Nijmegen late British mirror omitted by Glasbergen and De Laet on a chronological quibble, the Gundestrup-like Helden (Dutch Limburg) silver disc[33] and the little known silver pedestalled vase from Neerharen (Belgian Limburg) whose form and vine decoration looks truly Celtic rather than Greek.[34] Peaceful prosperity amongst the intrusive warrior element is illustrated by the La Panne 'briquetage' works, the 'Red Hills' of Flanders.

In the 'Marnian' Hainaut district cemeteries such as Leval-Trahegnies and Mont Eribus continue throughout the La Tène period, the former containing at least two chariot burials. From this area come the Frasnes-lez-Buissenal torcs[35] of 2nd century B.C. date but associated with Celtic mid 1st century B.C. coins—perhaps heirlooms buried during the advance of the legions.

Amongst the 'éperons barrés' of the Ardennes although poor in La Tène material, currency bars occur at Eprave (Nam.); Olloy and Hastedon (perhaps 'oppidum Atuatucorum' sacked by Caesar) are claimed as 'muri gallici' despite the absence of iron nails.[36] The complete lack of evidence for the heavy plough is interesting. La Tène settlements such as Santpoort (N. Holland) recently excavated by Dr P. J. R. Modderman display simple cross-ploughing marks and there seems to be no contradictory evidence from the south of the Low Countries. In this final period De Laet and Glasbergen's treatment of the Belgic problem is somewhat at variance with what Radford[37] and others have taught us. The Belgae proper settle in the 2nd century B.C. only south of the region which now bears their name; the single pedestalled urn from the Mont Eribus cemetery is considered insufficient evidence. Belgic status is thus denied to both Nervii and Menapii to whom are tentatively ascribed the La Panne workings, while the transrhenish Eburones between the Rhine and the edge of the Ardennes are of such antiquity that Caesar's term 'Germani' may here be of significance. It was the Eburones who were deported as part of Caesar's campaigns of 57 which mark the end of both British and Dutch prehistory.

[33] For the Black Sea parallels which support an East European origin for Gundestrup *contra* O. Klindt-Jensen in *Aarbøger* (1952), 209–11, see P. Goessler in *IPEK* (1929), 45–50 and Taf. 4.

[34] Professor Glasbergen has kindly sent me photographs of the vessel (now in Leiden); I hope to write further on this piece.

[35] The best publication of these splendid objects now in New York is contained in the Snettisham report; R. Rainbird Clarke in *PPS*, XX (1954), 42–94 and pls. V–VII.

[36] M. A. Cotton in *ANL*, 6:8 (1958), 194.

[37] *PPS*, XXI (1955), 250–2.

Notes and News

A GOLDEN STATUETTE OF A CELTIC WARRIOR PLATE V

Celtic warriors were popular in Hellenistic art, and even in Northern Italy where people had lived in contact with warriors from the north for years, these were still depicted in the Hellenistic manner. This can be well seen for example on the frieze of Civitalba, where the Celts are naked and carry their characteristic shields with a boss, of the so-called La Tène II type.

A short while ago I was shown a little golden statuette which had been sold to Mr Thomas F. Flannery Jr in Chicago from a big south European collection. The statuette was soldered to a brooch of fine workmanship, but it was evident that they did not belong originally to each other, and it is my impression that the pieces were combined fairly recently. Together with the warrior is a little lion which attacks him, and he defends himself with his sword and shield, his head being protected by a helmet. All these weapons are of types which fit well in the La Tène II period. The boss on the shield is like those from Civitalba, the sheath end ornament and the upper part of the sword with two projections at each side are just like those on typical La Tène swords (cf. ANTIQUITY, 1959, 162), and, finally, the helmet is a genuine Celtic type. There can hardly be any doubt that this statuette and the lions were made in the La Tène II period and perhaps in northern Italy as was the contemporary frieze from Civitalba.

OLE KLINDT-JENSEN

Mr J. M. de Navarro adds this comment: The general scheme of the brooch and the beasts, especially the heads on the catch and on the bow's foot facing the warrior, look to me earlier than the armed figure of the latter, whose helmet and shield boss (especially the former), should be not earlier than Middle La Tène. The beast heads must ultimately have been inspired from the east. My impression (from photographs only) is that the brooch might date from the 4th century B.C. and the figure not before the latter half of the third or the 2nd century B.C., i.e. that it was added later.

AN EARTHWORK AT SUTTON HOO

When the first report on the Sutton Hoo ship-burial appeared my curiosity was aroused by the strange course followed by the 100 ft. contour, about 600 yards south-west of the group of burials.[1] This suggested the presence of a broad bank forming part of the west and south sides of a large rectangular enclosure, the east side being hinted at by a small kink in the contour 250 yards to the east; but the shape of the contours could have been the result of other forms of disturbance, as for example gravel digging.

My curiosity had to remain unsatisfied for some time. No opportunity arose for an examination of the site at a suitable time of year, either by me or so far as I know by any of the archaeologists to whom I mentioned the matter, until March 1960, when I was able to pay a brief visit. The presence of substantial banks where indicated by the contours was confirmed, though the enclosure was not, as expected, rectangular. And although the remains

[1] ANTIQUITY (1940) 7, (fig. 1). The outline is also visible on the 1-in. O.S. Map, 7th Series, Sheet 150, TM (62) 284482.

are not in themselves spectacular their close proximity to the ship-burial and other barrows suggests interesting possibilities and seems to justify a note here recording their existence.

The plan of the banks so far as they could be followed up is shown in FIG. 1. It must be emphasized, though, that this is merely a sketch plan, made by a pace-and-prismatic-

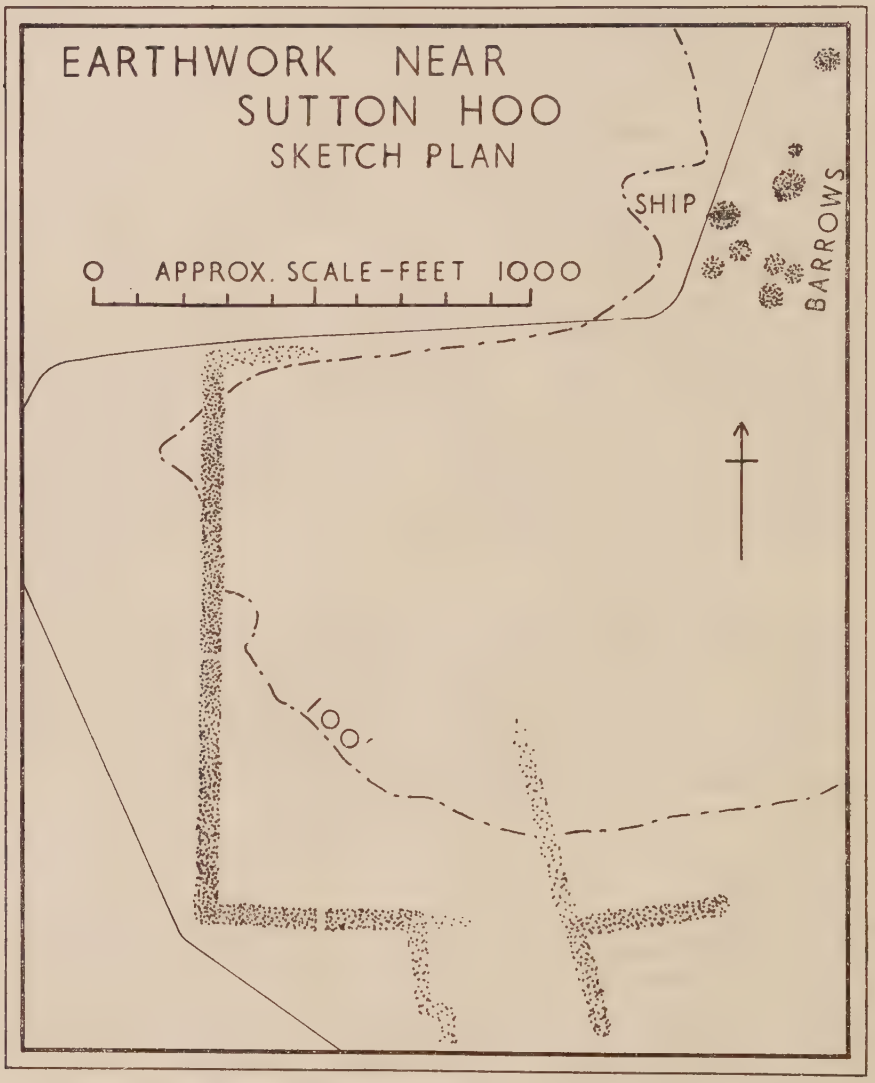


Fig. 1.

compass traverse; not much time was available, and work was almost limited to following the circuit of the earthworks; and the dead bracken was still high enough to conceal features of low relief. An accurate survey would require extensive clearance of the bracken, and there is little doubt that this, together with a more prolonged search, would reveal additional banks. No attempt has been made to show plantations, or trenches cut during the last war.

The earthwork was first located where it meets the modern fence running west from the barrows. It seems that there was a corner here, and that the fence, which runs along the top of a fairly steep natural slope, also coincides roughly with the bank, but this cannot be traced far to the east. To the south, however, it is clear and well-preserved. It first appears about 50 ft. wide by 3 ft. high, and runs almost due south for over 400 yards, increasing to nearly 60 ft. wide and 5 ft. high near the angle at its south end. It is much worn down, and is perhaps slightly higher on the west than on the east, but there is no sign of a ditch.[2] The gap half-way along this side may be ancient, but is more probably a break-through.

The corner appears to have been sharp; it was certainly not boldly rounded as in Roman forts. The bank continues east for about 160 yards, with a gap (apparently original) at 80 yards from the corner; it is not so well preserved, but still definite. The traces then become more obscure, and were overgrown with thick bracken, but they seem to follow the course indicated on the sketch. No definite junction with the T-shaped section of bank further east could be made out.

The south and east arms of this T-shaped section were clear, though overgrown, and seemed to end where indicated. The arm extending to the north-west, however, seemed damaged, and finally, at about 130 yards from the junction, it died out and could no longer be followed on the ground with any certainty.[3] It should be noted, though, that the crest of the ridge when viewed against the sky showed a slight rise approximately on the same line. It was not clear whether this area had ever been ploughed, as the bracken prevented observation. If, as seems probable, the bank formed a complete enclosure, this would have been trapezoidal in plan, containing about 17 acres, with the surface falling very gently in all directions from near the middle of the east side.

It remains to consider the function of the earthwork. It is clearly ancient; and it is almost certainly not a Roman military work or a fragment of an early field system. This leaves a wide range of possibilities, from an unusual type of Iron Age structure to a medieval park pale. But the proximity of the Barrows, which seem to be the only other early remains near by and which are in clear view from the north half of the enclosure, suggests at least a possibility that it is a domestic site to be associated with them.

A. H. A. Hogg

[2] It will be recalled that the ship-barrow was found to have been built up of turves, not with spoil from an accompanying ditch.

[3] This side, when plotted, was found to coincide with the east kink in the 100 ft. contour already mentioned.

PENNY WHISTLES AND PREHISTORY: Further Notes. PLATES VI(*b*) and VII

My article in ANTIQUITY XXXIV (March 1960) was in the way of being a bait for further information on musical instruments in prehistoric Europe. Not only have I had a nibble or two but the Editor has sent me a recent paper with some important additional material from Russia and Eastern Europe.[1]

The Mesolithic was rather a gap in my chronological survey and although Häusler has rejected as natural the three holes of the Pernau bone, he notes a Russian group which starts in the Late Magdalenian with an elk horn pipe from Moldova V on the bank of the Dneister pierced with four holes on one side and two on the other. This is followed by another four-holed bone from Chernaya Gora, central Russia. Amongst pipes from Kitoi period graves around Lake Baikal—termed 'Neolithic' by Okladnikov but strictly belonging

[1] A. Häusler, 'Neue fund steinzeitlicher Musikinstrumente in Osteuropa', *Wiss. Z. der Martin-Luther-Universität Halle-Wittenberg*, IX, 3 (1960) 321–32 and full references.

to a simple Mesolithic hunting and fishing economy[2]—are three unequal and fragmentary tubes from a grave on Zhiloi Ostrov dated to the end of the 3rd/beginning of the 2nd millennium B.C.[3] This find would seem to be a fair candidate for a series of 'pan-pipes' which Seewald noted as commencing with four bone tubes found with Aurignacian/Magdalenian polished scrapers at Le Placard, Charente.[4] At the other end of the scale are such objects as the Late La Tène five-reed pipe from Klein-Kühnau, Dessau.[5] From an 'ochre-grave' culture burial (at Mariupol near Kiev) is a fine example in bone with at least seven sections of graded size.[6]

Häusler also disposes of a series of geometrically decorated bone tubes from various 3rd millennium sites in the Aegean and the Near East as definitely being knife handles or pigment containers[7] although I think my example from Troy may still be allowed a place in the orchestra. Decorated bone tubes being in my mind, in glancing through the Editor's volume on the chamber tombs of France, my eye was caught by one pierced with a single hole. This was found as part of the grave goods in a short gallery grave, 'Le Dolmen du Terrier de Cabut', Gironde. The other objects included a Western European bronze tanged dagger, three segmented bone beads, and possible SOM. ware.[8]

For the Bronze Age in Britain Mr Nicholas Thomas has reminded me of some notes of his on Wessex Early Bronze Age grave groups in which Mrs Jean Jenkins comments on both the Normanton pipe and a possible bone horn from Barrow 18, Wilsford.[9]

Standfordbury has cropped up again as Dr G. H. S. Bushnell tells me that there are some practical points against Mr MacGillivray's reconstruction. On the other hand the Cambridge collections contain some other fragments of unknown provenance of those so-called flutes. Dr Bushnell writes: 'In 1953 . . . Dr D. E. Owen of the City Museum of Leeds . . . said he thought at least four instruments were represented.' Also Mr Richard Reece has sent me drawings of a similar though undated collection of seven sections of polished and turned bone, now in the Corinium Museum, Cirencester, while the British Museum possesses a single section of bone pierced with two holes found somewhere in the City.[10] When in the Netherlands last September I noted a series of objects which seem definitely to confirm the non-musical nature of this strange group (PLATE VI(*b*)). In the archaeological collections at Maastricht amongst a group of finds from the late Roman

[2] A. P. Okladnikov, *MIA*, 18 (1950), 396 ff.

[3] A most useful summary of this material is H. N. Michael, 'The Neolithic Age in Eastern Siberia', *Trans. American Phil. Soc.*, N.S. 48, Pt. II (1958); here 69–70, fig. 66 and references. My thanks are due to Professor Stuart Piggott for bringing this publication to my notice.

[4] O. Seewald, *Beiträge zur Kenntnis der steinzeitlichen Musikinstrumente Europas* (1934), 36 f.

[5] Häusler, *op. cit.*, 323 and Taf. III, 4. F. Behn, *Musikleben im Alertum und frühen Mittelalter* (1954), 110 f. notes the importance of the pan-pipe to the development of music and at Taf. 193 illustrates the better known wooden example from Alesia.

[6] Michael, *loc. cit.*

[7] A handy English reference is M. W. Prausnitz, 'Ay and the chronology of Troy', *11th. Ann. Report Inst. Arch.* (1955), 21 f. and fig. 2.

[8] Glyn Daniel, *Prehistoric Chamber Tombs of France* (1960), 136 f. and pl. XXIII. This may be added to the antler pipe from near Poitiers which I noted in my article. In contrast an unpierced decorated bone was found in the Grotte de la Treille, Narbonne, with faïence beads; Daniel, *op. cit.*, 144.

[9] *WAM*, LV (1954), 323, fig. 3; Mrs Jenkins had also written a handy introduction to primitive musical instruments issued by the Horniman Museum; *Musical Instruments* (1958). A continental Bronze Age newcomer (of pottery!) is published by O. Seewald, 'Eine bronzezeitliche Gefässflöte von Vörösmart, Kom. Baranya, Ungarn', *Acta Praehistorica*, II: Centro Argentino de Estudios Prehistorios (1958), 111–21. Unfortunately at the time of writing I have been unable to follow up this reference kindly supplied me by Dr K. Willvonseder of Salzburg.

[10] J. W. Brailsford, *British Museum Guide to the Antiquities of Roman Britain* (1951), 78, fig. 40, 4.

cemetery under the cathedral of St Servaas are four sections of bone turned, grooved, and drilled in the manner of our English examples. In addition there are a number of carved bone baluster-shaped pins carefully made to fit at either end into the drilled holes. Together with a number of circular bone washers, this odd assortment smacks more of spillikins than shamanism, but Mr Reece has a rather more constructive suggestion. He writes: 'In the Museum in the Hôtel de Sade at St Rémy, Provence, amongst the finds from the excavations at the site of Glanum are not only about sixty of these bone objects, but also a series of exact wooden replicas. Found in waterlogged ground these pieces of wood are still recognizable, although shrunken, and, most important, are still threaded on a piece of twine. This suggests that they were suspended on the string and meant to move along it in the manner of an abacus, which is indeed the conclusion to which the museum authorities of St Rémy have come. With this evidence from a wooden model it seems very likely that the English bone counterparts are meant for the same purpose.'

On a less mysterious tack another result of my Netherlands visit is that in conjunction with Mr J. D. van der Waals of the Biologisch-Archaeologisch Instituut, Groningen, I plan to make an extensive musicological study of the large series of bone pipes from the pre-Roman levels of the Dutch *terpen*. Such an examination will, we hope, lay the basis for a serious discussion of the tonal system (or systems!) of north-western Europe in pre-classical times.

Finally, I was very glad to read Dr Hutchinson's notes on *auloi*, but with due deference to the Editor I must underline that such pipes as the one shown on the side of the Hagia Triada sarcophagus are by no means 'penny whistles' but must represent reed instruments of the oboe family.[11] One could also quote a Nuraghic bronze figurine in the Cagliari Museum who plays the same type of double shawm as was recovered from the Avar burial site at Jánoshida.[12] Coming full circle the fine photograph by Mr Doyle of Newquay (PLATE VII) shows a 20th-century Cretan playing on a bone penny-whistle.

J. V. S. MEGAW

[11] R. W. Hutchinson, ANTIQUITY (1960), 140–41. Mr MacGillivray and I illustrated this difference in a broadcast in *The Archaeologist* series, 'Prehistoric Musical Instruments and their Music', B.B.C. Network Three, 12 April, 1960.

[12] D. von Bartha, 'Die avarische Doppelschalmei von Jánoshida', *Archaeologia Hungarica*, 14 (1934); Mr Lindsay Langwill of the Galpin Society reminded me of this reference. See also A. Baines, *Bagpipes*; Pitt Rivers Museum Occasional Papers on Technology, 9 (1960), 51–3, 58 and fig. 24.

NEW LINEAR B TABLETS FROM MYCENAE PLATE VI(*a*)

During excavations undertaken within the citadel of Mycenae in August and September, 1960 under the joint direction of Dr John Papademetriou of the Greek Archaeological Service and Lord William Taylour fragments of eight Linear B tablets were brought to light. These are of particular importance as they are the first ones to be found on the acropolis itself. Previous finds of tablets at Mycenae have come from houses outside the citadel, which were excavated by Professor Wace and more recently by Dr Verdelis, the Ephor for the Argolis.

The area at present being excavated lies just beyond the Grave Circle. It is bounded by the South House and the Ramp House to the north and Tsountas's House to the south. On the east there is some terrace walling and from here the ground slopes down to the Cyclopean fortification walls, the western limit of the excavation. It was in clearing a corridor-room in between the terrace walling and the store-room excavated by Professor

Wace in 1954 that seven of the tablets were discovered; a further tablet was found close by just to the north of the store-room. All of these fragments, for none of the tablets is complete, were embedded in a hard concrete-like substance (PLATE VI(*a*)), the result of a fire of exceptional intensity that had fused stone, mudbrick, and rubble into a solid mass. This destruction debris had fallen from buildings on the upper terrace and therefore the tablets do not belong to the corridor-room, which has just been excavated; furthermore they were found comparatively high up in the debris. Tablets excavated under such conditions are not easy to recognize and this may account for the failure of earlier excavators on the acropolis to find them, particularly if they had no occasion to expect them or be on the look-out for them.

The fragments are mostly of fair-size tablets. Four of them are quite well preserved and and legible; one of them is blank except for the lines used for dividing off the script. Like so many of their predecessors they appear to be inventories. They are at present being studied by Mr John Chadwick.

W. D. TAYLOUR

PRESERVING AN ANCIENT CHURCH—IN A GLASS CASE PLATE VIII

A remarkable effort is being made in Hungary to preserve the ruins of a 13th-century church.

The church is at the village of Zsámbék, some twenty miles out of Budapest. In 1186 the village was given by King Béla the Third to a member of the French Aynard family who served in his queen's suite. A descendant of the family had a church and convent built there about 1258 and gave it to the Premonstratensian Order. The building is a fine example of Romanesque-Gothic style. French art historians hold that Villard de Honnecourt, the genius of 13th-century French architecture, had a hand in it. Basilica-shaped, its finely carved ornaments are Romanesque, but the big stone rosette on its façade is Gothic, Its vaulting has much in common with several old churches in Normandy.

The building weathered the stormy history of Hungary, though it was badly battered in the process. In 1475 it became the property of the Order of St Paul. In the 16th century it became a fortress of the Turks. Its ruin probably dates from 1581 when the Hungarians tried to recapture it from the Turks. After the Turks left Hungary it was further damaged in the 1763 earthquake. For nearly 200 years the weather and the depradations of local builders made further inroads on the ancient structure. It is said, with little exaggeration, that if all the shaped stones could be taken from the small houses of Zsámbék the building could be restored in its original form. A couple of years ago an article in a newspaper drew attention to the need to preserve what remained, and as a result serious restoration work began in 1959. The project attracted the enthusiastic support of art historians, architects and professors of various faculties of the Budapest Technical University. Experts of the National Society for Preservation of Historic Buildings took the lead.

Restoration will be in two stages. First, as much as the stonework as possible will be put back into place or made safe. Cracks or fissures in the masonry will not, however, be filled in, as the main principle of the work is to preserve the ruin-like character of the building. The second part of the job is the most ingenious. It is the idea of Professor Frigyes Pogány, of the Faculty of Modern Architecture at the Technical University. He intends to case in the entire nave with glass, set in very light metal framework, to preserve the restored fabric from further deterioration. Work on this will commence when the restoration of stone work is completed. (PLATE VIII)

LAWRENCE KIRWAN

POTASSIUM-ARGON DATING

Mr Barker of the British Museum Department of Research Laboratory writes (20 *October*, 1960):

I feel that some comment is necessary in respect of certain statements in Dr C. B. M. McBurney's note: 'Potassium-Argon Dating' in the September issue of ANTIQUITY (pp. 213–15).

The author, in a reference to radium dating, makes the following statement: 'In the case of *radium* (my italics), the half-life is so immensely long that it is remotely unlikely that it will ever yield results which can be measured in units of less than a million years.' This is clearly incorrect, since, in fact, the half-life of the principal (and longest lived) isotope of radium (Radium 226) is approximately 1600 years. However, it is obvious from his reference to the work of Professor Holmes that Dr McBurney probably has in mind the lead-isotope method of dating which is based on the radioactive decay of uranium. In this case, the above comment on the half-life would be quite acceptable since the half-lives of the uranium isotopes U 235 and U 238 are $7{\cdot}1 \times 10^8$ and $4{\cdot}5 \times 10^9$ years respectively.

The term radium dating is usually taken to refer to the work of Urry *et al.*[1] on the radium content of deep sea sediments. This provides a possible method of dating covering the past quarter of a million years or so and is applicable to a variety of geophysical, climatological and oceanographic problems.

Dr Charles McBurney comments: I am grateful to Mr Barker for calling attention to the two errors in my note; he is, of course, correct in supposing that I had in mind the Uranium-Radium-Lead method of Professor Holmes (where the principal time-lag occurs *before* radium is produced) and not the less well-known method of Urry. The former, as I intended to make clear, is rendered impractical by the length of the half-life involved.

[1] Urry, Wm. D., *Amer. J. Sci.*, **240** (1942) p. 426; Piggot, C. S. & Urry, Wm. D., *Bull. Geol. Soc. Amer.*, **53** (1942), p. 1187; Urry, Wm. D., *Amer. J. Sci.*, **246** (1948), p. 689; *J. Marine Research*, **7** (1948), p. 618; *Trans. N. Y. Acad. Sci.*, **10** (1948), p. 63.

POTTERY ANCHORS

In the last issue of ANTIQUITY Mr D. H. Trump suggests that these are not anchors but hooks used in weaving, basing his theory on 'the worn grooves which could only be caused by thin threads looped over the hooks and sawn back and forth'. He notes their association with 'loom-weights and spindle-whorls'. But in archaeology *all* the known evidence should be taken into account. Mr Trump has not noticed the significance of the sites where these objects were found. He mentions four; the mainland of Italy (one example only), and three islands, Sicily, Lipari, and Malta. In a small rocky island like Malta, where the depth of soil except in the valleys is too shallow for agriculture the staple food must have been fish. A large proportion of the inhabitants must have been fishermen. The sites in Malta where these anchors are found are also significant. There are only two, Borg en-Nadur which is close to the only harbour on that grim inhospitable south coast, and Bahriya, which overlooks a small inlet on the south-west of the narrow channel between the islands. None of these objects is found in the central part of the island nor along the sandy beaches of the north coast as one would expect if they were connected with weaving and spinning, which were women's work in the Bronze Age. But if they were connected with the fishing industry, there might well be a reasonable explanation. In those fierce and sudden gales, for which the Mediterranean is noted, a fishing boat or a small trading vessel driven by the wind along the south coast could find safety from the precipitous cliffs and treacherous

reefs only in St George's Bay (Borg en-Nadur) or in the inlet of Bahriya. The crews might well be sufficiently grateful to the Deity to whom they owed their safety to present to his shrine an anchor which to all seamen is the emblem of safety.

To turn to the 'loom-weights' and 'spindle-whorls'. The looms of the Bronze Age were of the horizontal type, for which weights to keep the warp threads taut are not necessary. But for fishermen net-sinkers are required and are essential. The spindle-whorls are in every case too heavy for spinning the 'thin thread' that Mr Trump envisages, but they could be adequate for the twine with which fishing nets are made. It is noticeable that most of these objects were found within the precincts of a sanctuary. It is a not uncommon belief that objects made or dedicated in a holy place bring luck to the user, and as fishermen are often in danger they would have faith in objects that had been blessed in this way. It seems to me that unless the whole of the background is carefully studied with all its implications, including the chronological position, the archaeologist, when confronted with an enigmatical object, is in danger of becoming a mere technician.

In conclusion I should like to point out that this is not the first time that my identification of the 'pottery objects' as votive anchors has been challenged. The *Mariner's Mirror* published my best specimen as one of the earliest examples of an anchor with flukes. This brought an indignant remonstrance from a well-known German classical archaeologist, claiming that if the object were placed with the 'flukes' pointing down, it was clearly a *thyrsos*. We now have three choices as to the meaning of these objects: (1) votive anchor, (2) *thyrsos*, (3) variant of single hook used in weaving during the Iron Age (how many centuries later?) in Greece.

M. A. Murray

SUDANESE POTTERY IN ENGLAND: A WARNING

Early in 1949 some sherds, said to be Saxon, found under a hard tennis-court in the garden of 'Applegarth', Green Lane, Stanmore, Middlesex, were shown to Mrs H. E. O'Neil. Her enquiries soon confirmed that they were not Saxon, but it was not until the end of the year that the material was identified by Mr A. J. Arkell as coming from Jebel Moya, about 150 miles south of Khartum in the Sudan.

On further enquiry Mrs O'Neil discovered that the sherds had been discarded from the Burroughs Wellcome store at Willesden which from 1937 onwards housed the finds from the Wellcome excavations in the Sudan. Mr (later Sir) Henry Wellcome had insisted during these excavations that every scrap of material should be 'conserved for study'[1]; the resulting mass of finds was so great that Dr F. Addison found it necessary when he came to sort the collections at Willesden to adopt a 'policy . . . of ruthless but not indiscriminate discarding' by which the material was 'considerably reduced in volume'.[2]

The discarded material was bought up by Mr Henry Burn of Stanmore, a landscape gardener, as foundation debris for garden paths and it was he who laid out the garden at 'Applegarth' just before the War.

Here the matter rested until October 1953, when the writer was called to look at some pottery found in the garden of 51 Wolsey Road, Moor Park, Northwood, Middlesex. Several hundred sherds, both decorated and plain, were collected on the north side of the garden. It soon became clear that they were not at home in any known British context and a letter to the late Mr B. H. St J. O'Neil resulted in their identification some time later as a

[1] O. G. S. Crawford, *Said and Done* (1955), 102; F. Addison, *Jebel Moya* (1949), 1, 199.
[2] *Ibid.*, 1, 9, 199.

further selection of the discarded Jebel Moya material.[3] The then owner of the house, Mr John Rae, had himself laid out the garden and knew nothing of the pottery until he had turned it up in a flower-bed. How it came to be in Moor Park is thus quite unknown. It is clear however that the contractor's undertaking to put the material under a steam-roller had not been carried out and that the pottery had been dispersed over a fairly large area in north-west Middlesex.

Mr O'Neil suggested that a general warning to this effect should be issued and this note has been written in the hope that other strange finds of this sort from the London area may cause fewer worries in the future than they have done in the past. The problem of where to put discarded material often arises: the best answer is to return it to the site whence it came; the only practicable solution may often be to send it to the municipal rubbish-dump. There it is at least securely stratified with 20th century products![4]

MARTIN BIDDLE

[3] The Stanmore sherds were identical with those illustrated in *Jebel Moya*, II, pl. XCV, D, 1–7; the Moor Park sherds are paralleled in pls. XCIV–CIII, *passim*.

[4] The writer is greatly indebted to Mrs Helen E. O'Neil for placing her notes at his disposal and for permitting him to refer to some of Mr O'Neil's letters.

FRENCH PREHISTORIC CONGRESS, 1961

The XVIIth session of the *Congrès Préhistorique de France* will take place in Brittany in September 1961 under the presidency of Dr P. R. Giot. The provisional programme is as follows: 4 September, opening session in Rennes, 5–7 September, working sessions in Rennes; 8 September, excursion Rennes—Brest; 9–10 September, excursions from Brest; 11 September, working session of the Atlantic symposium in Brest and closing session; 12 September, excursion Brest–Quimper; 13–15 September, excursions in the Morbihan. Three special questions have been put on the programme of the working sessions, namely (1) the Western Neolithic and the Neolithic Cultures of the Atlantic countries, (2) the Atlantic Bronze Age cultures, and (3) Relations between the Iron Age cultures of Iberia, southern France, Armorica and Cornwall. The Atlantic Symposium in Brest will be a multilingual conference about all these problems pertaining to the countries on the Atlantic seaways. We are asked to say that during one or two of the working sessions in Rennes, when all the papers will be in French, additional excursions can be arranged for those interested in seeing more sites. For details of membership write to: the *Société Préhistorique Française*, 250, *Rue Saint Jacques*, *Paris V*, enclosing an International Reply Coupon.

MORE ON THE MESOLITHIC

A recent pair of notes in ANTIQUITY has discussed the origin of the word 'Mesolithic'. Miss Judith Wilkins[1] has shown that Hodder Westropp seems to have been the first to use the term, in a paper to the Anthropological Society in 1866.[2] Dr Carl-Axel Moberg[3] agrees that the priority which general Swedish belief[4] had attributed to Professor Otto

[1] ANTIQUITY, (1959), 130–31.

[2] 'On the Analogous Forms of Implements among Early and Primitive Races' (*Anthropological Review*, 1866, p. clxxxiii, and *Memoirs read before the Anthropological Society of London*, 1866, vol. II, pp. 288–93). Curiously enough, the term Mesolithic seems to have vanished from the version of this paper published in his 1872 book *Pre-Historic Phases, or Introductory Essays in Pre-Historic Archaeology*, though it occurs elsewhere in the book, vid. p. 65 and the table facing p. 1.

[3] ANTIQUITY, (1959), 220–1.

[4] Recently expressed by Nils Niklasson 'När och av vem Präglades Uttrycket Mesolitikum?' in *Fornvännen*, 1955, pp. 46–9.

Torell of Lund at the International Congress of Anthropology and Prehistoric Archaeology, 1874, now appears to belong to Westropp; but, as he points out, the interesting thing is that Westropp was not the only one to use the term before J. Allen Brown gave it an approximation to its modern usage in 1892.

Given the terms Palaeolithic and Neolithic, introduced by Lubbock in 1865, it was almost inevitable of course that a 'Mesolithic' would sooner or later be suggested by interim —or transition-minded workers. And there are more instances than Torell and Westropp to illustrate this, from places as far removed as India, Argentina and France. At the *Congrès International d'Anthropologie et d'Archéologie préhistorique* in Bologna, 1871 (i.e. three years before Torell used the term at the Stockholm Congress), a French prehistorian, J. Reboux, offered a paper, 'Recherches paléontologiques dans le Bassin de Paris', in which he divided the Stone Age into *Paléolithique*, *Mésolithique* and *Néolithique*. The *Mésolithique* was the period when flint was shaped into axes, and implements were fashioned in antler, shell, bone and wood; in addition, reindeer were replacing elephants, horses, cattle and rhinoceroses. He illustrates the industry with what seems to be a retouched Levallois flake from Clichy la Garenne (Fig. 2, p. 100 of the Congress papers). He followed up this paper with several more in 1873 and 1874 along much the same lines,[5] emphasizing the technological side of the term: the Mesolithic was a method of stone working by fashioning the nodule or pebble in circular or oval form by retouch along the edges, making an elongated axe with two cutting edges. Reboux's concept of an intermediate technological stage does not seem to have caught on, although the hiatus was one of the burning issues of the day in France; perhaps because of his obvious mélange of palaeontological and archaeological data, perhaps through the hostility of Gabriel de Mortillet and others who opposed the idea of a transitional stage between the Palaeolithic and the Neolithic in France.

A few years later the Argentine palaeontologist and prehistorian, Florentino Ameghino, presented a memoir at the *Congrès International des Sciences Anthropologiques* in Paris, 1878, which dealt with prehistoric man in the La Plata basin and used the terms Palaeolithic, Mesolithic and Neolithic.[6] For him the Mesolithic was the intermediate period, without pottery but with much worked bone, in post-Quaternary times with a nearly modern fauna. Ameghino's usage of the terms Palaeolithic and Neolithic in his New World sequence was certainly adopted from the current European schemes, but it is not clear whether his 'Mesolithic' was also borrowed rather than invented independently.

As Miss Wilkins has already mentioned, most modern writers have considered that J. Allen Brown invented the term in his paper to the Anthropological Institute in 1892,[7] although it was struck down shortly after by Boyd Dawkins. But although Miss Wilkins is right, as far as his 1892 publication is concerned, in stating that 'Brown's phrases do not seem to indicate any suspicion that he had been anticipated', it is perhaps worth pointing out that Brown himself, in an earlier article, had recognized his debt to yet another predecessor. In a paper given to the Anthropological Institute on 27 March, 1888[8] discussing the microliths and geometrics found in Asia, North Africa and Europe, he says that his friend Mr A. C. Carlyle, 'late of the Archaeological Survey of India', had proposed calling them *Mesolithic* in India on the grounds that they connected up the

[5] 'Démonstration de l'Emmanchure des Instruments des trois Epoques de l'Age de la Pierre' (*Mémoires de la Société d'Ethnographie*, Paris, vol. 12, part 2, 1874, pp. 225–31); 'Chronologie de la Pierre' (*Bull. Soc. d'Anth. de Paris*, vol. VII, 1873, pp. 523–31).

[6] 'L'Homme préhistorique dans le Bassin de la Plata' (*Congrès International des Sciences Anthropologiques*, Paris, 1878) (1880), pp. 341–50.

[7] 'On the Continuity of the Palaeolithic and Neolithic Periods', *JAI*, XXII, no. 1–2, 1892, pp. 66–98.

[8] 'On some highly specialized Forms of Stone Implements found in Asia, North Africa and Europe', *JAI*, XVIII, 1888–9, pp. 134–9.

Palaeolithic and the Neolithic there. It is not known to the present writer whether Carlyle ever published this idea at any length, and probably Brown got it from him privately when Carlyle exhibited a flint collection from the Vindhya Hills of Central India at the Royal Albert Hall in 1887.[9] But the real point here is that Brown received the notion from another, and perhaps entirely independent, coiner of the expression in the period between 1866 and 1892.

It is more than likely that the three names mentioned in this note do not exhaust the list of people inventing or re-inventing the term during his period. Recently an American anthropologist, Professor Wilson Wallis, has stated that he had known in England in the early years of this century a 'nonacademic enthusiast' named A. M. Bell who had proposed the designation 'Mesolithic'[10]; but Bell seems to have published little or nothing on this subject, and may well have been following up J. Allen Brown's idea after Piette's work at Mas-d'Azil became known. However this may be, the numerous re-inventions of the word would have seemed quite natural to Westropp, with his belief in the psychic and intellectual unity of mankind whereby similar wants combined with 'the same natural instincts and spontaneous powers of suggestion'[11] would inevitably produce similar forms anywhere in the world.

PHILIP E. L. SMITH

[9] G. de G. Sieveking has recently published some further biographical details on A. C. Carlyle, and it seems that in the *Catalogue of the Exhibition of Paintings, Sculpture, Indian Antiquities, etc.*, published in 1887, certain of the flint collections were described in Section V as Palaeolithic and Mesolithic. ('Morhana Pahar: or the Mystery of A. C. Carlyle', *MAN*, vol. LX, 140, July 1960, pp. 98–100.)

[10] 'Anthropology in England early in the present Century', *American Anthropologist*, vol. 59, no. 5, 1957, p. 788.

[11] Westropp, *op. cit.*, 1872, p. 115.

C.B.A. CONFERENCE ON THE BRITISH BRONZE AGE, 1960

The Neolithic and Bronze Age Research Committee of the Council for British Archaeology organized a conference on Problems of the British Bronze Age in London in December, 1960. Dr John Coles, Assistant Lecturer in Archaeology at Cambridge, reports on the conference.

It had been planned to hold this conference in the Institute of Archaeology, University of London, but the demand for tickets was so great that the gymnasium of University College had to be employed, and this was packed to overflowing for several of the lectures. An exhibition of 'Some Sites and Relics of the British Bronze Age' was held in the Institute of Archaeology during the week 12–18 December; this included recently excavated material, some new hoards, and comparative material loaned by the Instituut voor Prae-en Protohistorie, Amsterdam.

The opening talk on Friday evening, 16 December, gave the audience an opportunity to hear and see Professor C. F. C. Hawkes at his best. His 'Scheme for the British Bronze Age', covering a range of some 1500 years, seems likely to remain a basis for some time to come, although minor details probably will require some rearranging, as Professor Hawkes himself expects. The major divisions, rather than purely typological, are now based upon technological processes as well. A Copper Age, from about 1850 to 1650–1600, includes Beaker material and some local industries. The spread of knowledge of bronze in Britain comes around 1650 with Continental quest for tin and gold, and the Battle-Axe element is emphasized. From this date our industries, both British and Irish, develop, and central European contact with Brittany and southern England sees the rise of the Wessex culture. Cornish tin then begins to be exploited and trade routes and links provide a basis for Wessex 2, with Highland British and Irish connections.

The Middle Bronze Age begins *c.* 1400 with the emergence of a single Insular bronze culture in Britain and Ireland, and Professor Hawkes designates five provinces, which may cause some difficulty unless they are treated merely as geographical units as intended. The cremation rite is practised throughout the provinces, and the metal industrial leadership passes to the north and Ireland for the production of insular types. This continues throughout the Middle Bronze Age, although north European types and regional developments occur, especially in the south. The final phase of the Middle Bronze Age, down to about 850 B.C., now has late local types including the earliest British-produced swords. Thus the old theory, that the Late Bronze Age begins with the appearance of the leaf-bladed sword and spearhead, and the socketed axe, has been eliminated, as all three have been shown within the last dozen years to begin in or run concurrently with the Middle Bronze Age.

The traditional beginning of the Late Bronze Age, 1000 B.C., has been reduced to about 850. Professor Hawkes's Late Bronze I is typified by the 'Wilburton complex', and runs on minimum reckoning for something less than 100 years. It is placed in the scheme as Late Bronze I mainly because of sword typology, although there are some rare links with early Hallstatt B of central Europe. But the new Isleham hoard (ANTIQUITY, 1960, 279), with Wilburton elements, also contains bucket fragments which designate Professor Hawkes's Late Bronze 2. If the south-east English carp's-tongue sword complex and the Covesea incursion of north-east Scotland are relegated to minor status within Late Bronze 2, then possibly the Wilburton complex should not be granted a number of its own. Further work on continental links should resolve this.

In general, the scheme as set out by Professor Hawkes received widespread approval and it was only to be expected that the later papers in many cases reflected and confirmed his views.

The first paper on Saturday's session, by Dr I. F. Smith and Mr I. H. Longworth, was a valuable contribution to the study of Bronze Age pottery. 'The Neolithic Continuum' as shown by Bronze Age Ceramics traced the origin of the Collared Urn back into early Neolithic forms, and a full discussion by Dr Smith of the various contributions and evolutions of Ebbsfleet, Mortlake, Fengate and Beaker ware extended Abercromby's and Leeds's derivations well back into the 3rd millennium B.C. Mr Longworth, continuing the story, separated and discussed the primary features of Collared Urns, and thereby established a sequence pattern for Cinerary Urns to which then could be applied known datings, his early, primary, urns deriving from Fengate and Mortlake, his secondary series beginning in Wessex 2.

'A Statistical Consideration of the Wessex Bronze Age' was Professor R. J. C. Atkinson's contribution to the conference, and it was one of the most stimulating of the entire proceedings. The use of statistics in archaeological studies is, of course, not new, as various schemes have been in use in America and Europe for some time, but as far as I know, the particular method employed by Professor Atkinson is a new approach, as may also be the use of an electronic computer. The method, discriminate analysis, depends upon a basic assumption, in this case the theory that Wessex culture inhumations are early, cremations are later. Accepting this, then various schemes can be evolved, and a series of internal and external checks made, all of which tended to support the basic premise. The conclusions proposed by Professor Atkinson, that the Wessex culture can be divided into two stages, with a further subdivision of the later stage, are of extreme importance as now the full range of contents of each division can be seen. The question of the exotic elements in the Wessex culture, including amber spacers, faïence, gold-mounted amber discs, was fully discussed and the problems of their survival values explained. The method of analysis

used for this Wessex material proved its worth, but in general this basic assumption approach needs great caution to avoid a circle effect, and outside checks are required.

The next lecture, by Mr Charles Green, briefly described the results of some total and some partial excavations of Bronze Age Round Barrows near Shrewton, Wiltshire, and was followed by Professor John Evans who gave a general survey of contacts between the British Bronze Age and the Mediterranean. These included amber spacer plates, faïence and related beads, gold bound amber discs, gold cups and some daggers and double axes; they could be divided into two chronological groups, but the evidence for routes connecting Britain and the Aegean still was scanty and difficult to interpret.

Professor Stuart Piggott's paper, on the British Beaker cultures, entitled 'Abercromby and After,' dealt briefly with the Iberian origin of Bell Beakers before moving on to the British material. The Beaker colonization of Europe stemmed from the Iberian meseta and maritime beakers, Britain receiving its main types from the well-known reflux movement. The connections with early metal industries, Irish-Iberian contacts, and the more clearly demonstrable central European influences, were discussed and greatly clarified. It was interesting to see a number of Beaker tanged daggers illustrated and their local and continental features pointed out. After describing the local British Beaker types with their associations and origins, Professor Piggott closed an important paper by remarking on the Beaker influence seen in some late Neolithic pottery, as Dr Smith and Mr Longworth had demonstrated.

The final day's lectures began with a summary of Deverel-Rimbury and the Dorset Region, by Mr J. B. Calkin. He divided the pottery into three types, the bucket, barrel, and globular forms, and suggested that the last two were introduced into his region by invaders, possibly from Cornwall. The evolution of urn forms into the local Dorset bucket was described, via biconical and sub-biconical urns. Difficult here was the exact meaning of the word biconical, as different interpretations seem to have been applied by various people. Mr Calkin's paper served as a useful survey for the succeeding lecture, by Mr A. M. ApSimon, on 'The Trevisker and Deverel-Rimbury Cultures: Origins, Development and Chronology.' Mr ApSimon described the settlement site at Trevisker in Cornwall, both house types and pottery styles. These could be traced eastwards to Dartmoor and eventually to Dorset and the Deverel-Rimbury region. In the spirited discussion that followed this paper, Mr Charles Thomas argued vehemently against this Trevisker interpretation, and the general consensus of opinion seemed to be that the origins of the Deverel-Rimbury culture were still somewhat obscure.

The final paper was a general survey of the Final Bronze Age in the North and Scotland, by Dr J. M. Coles. This described Bronze Age developments from the 12th to the 4th century B.C., and supported the idea of a settlement direct from the continent in the 7th century, the Covesea incursion. Flat-Rim pottery was also discussed and several regional groups identified, although much further work is clearly required.

Following this, a General Discussion was held and some clarifying points made. Mr J. D. Cowen discussed in general terms the nature of evidence and its interpretations, and stressed the dangers inherent in pottery typology. The archaeologist must rely on non-ceramic evidence for a framework of dating and development, to which pottery styles could then be applied; the best example of this was Professor Piggott's work on Beaker associations. Professor W. Glasbergen and Dr P. J. R. Modderman illustrated and described a new find of a Hilversum urn associated with a Montelius II disc-headed pin, and forever disposed of the Deverel ghost. The presence of Professor Glasbergen, Dr Modderman, and their associates Dr J. J. Butler and Mr J. D. van der Waals, added greatly to the success of the conference.

Professor Hawkes then delivered the Closing Remarks, before the conference was officially ended by Dr J. N. L. Myres, President of the Council for British Archaeology. Professor Hawkes commented on the need for a chronology based on bronzes, the backbone of his Scheme, but the difficulty was that so few bronzes were revealed by proper excavation, whereas pottery sequences were more easily obtainable. He then briefly discussed some of the major problems remaining in the British Bronze Age, among which are Food-Vessels and cup-and-ring markings.

J. M. Coles

SUMMER SCHOOL ON 'THE ROMAN VILLA'

The 1961 British Summer School of Archaeology will be held at Cirencester from 12–19 August. The theme of the 1961 school is 'The Roman Villa' and the lecturers will include Mr H. C. Bowen, Miss Joan Liversidge, Professor I. A. Richmond, Mr A. L. F. Rivet, Dr D. J. Smith, and Mr Graham Webster. For full details of membership (fee for resident members and local members, *one guinea*; junior members, *half a guinea*) and accommodation, write to the *Honorary Director, F. T. Wainwright, Ingleby, Newport-on-Tay, Fife.* Application for membership should be made not later than 1 May, 1961. Dr Wainwright tells us that it is hoped that the 1962 Summer School will be in Brittany.

REVUE D'HISTOIRE DE LA SIDÉRURGIE

We draw the attention of readers to the *Revue d'Histoire de la Sidérurgie*, produced by the *Centre de Recherches de L'Histoire de la Sidérurgie*, under the direction of its Secretary-General, General Joseph Berlorgey, and the general editorship of Monsieur Bertrand Gille. This journal is published three times a year; the annual subscription is 23 N.Francs a year (*Hôtel de Ville, Nancy*). The third number of its first year of publication, 1960, is devoted to the hammer from earliest times to the modern steam hammer. An interesting contribution to industrial archaeology with a project for a *Musée du Fer*.

BRITISH ARCHAEOLOGY ABROAD

The first news has just reached us of the establishment of a British Institute of Persian Studies in Tehran during the coming financial year. The Institute is designed to provide a long-needed focus for British students concerned with any aspect of Persian archaeology, history, language, literature or the fine arts. The Persian Government and the University of Tehran have given much encouragement to the new foundation and the University is generously providing quarters for it. The first Director will be Mr David Stronach, and the Chairman of the Governing Council will be Professor M. E. L. Mallowan.

The recently founded British Institute of History and Archaeology in East Africa is now fully established, with main headquarters at Dar es Salaam. As originally planned, Mr Richard Goodchild's secondment to the Institute from the Libyan Government ends in August 1961 and he will be succeeded as Director on September 1st by Mr Neville Chittick. A long-term training-centre in field archaeology and excavation has been set up on the island of Kilwa Kisiwani, off the Tanganyika coast. The island is important historically and as the headquarters of a medieval mint, and has recently revealed also traces of a Stone Age culture. The Institute's first Nuffield Students are Miss S. V. Pearce, who is undertaking a study of Iron Age cultures under the direction of Dr M. Posnansky of Kampala, and Mr B. A. Ogot, who is recording and analysing the oral traditions of the Lwo group in northern Uganda.

Reviews

THE BRONZE AGE ROUND BARROW IN BRITAIN. *By* Paul Ashbee. *London: Phoenix House*, 1960, 50*s*. *pp*. 222, 61 *text-figures*, 32 *plates*.

The publishers on their wrapper sub-title this book 'The Bronze Age Revisited', which concisely indicates Mr Ashbee's achievement. Most of the raw material for a study of the material culture of those who buried their dead under the barrows or cairns of Bronze Age Britain has been in our museums, and imperfectly recorded in our archaeological literature, for generations; its interpretation waited upon many factors, of which the most important was the need for new scientifically conducted excavations which would not only produce evidence of structural features ignored by the earlier barrow-diggers, but would make it possible to re-assess old finds in modern terms. Mr Ashbee has revisited the Bronze Age armed with this new knowledge, much of it acquired at first hand in his own excavations, and here reports on what he sees. To some, the picture may look unconventional at times, but this is because it is, most happily, in a modern idiom, and the author has taken a realistic viewpoint based on the demands of the evidence itself. Above all, he has freed himself and his subject from the domination of uncontrolled pottery typology, which has done so much, since Abercromby's day, to bedevil the study of the British Early and Middle Bronze Age.

It is a work invaluable to students, and attractive and informative to the educated layman. The author begins with a short survey of the development of the interest in, and investigation of Bronze Age burials in this country, continues with the topographical and external content of the subject, and then proceeds to an analysis of the evidence for internal structure, grave types, burial rites, 'art', and the grave-goods themselves. This is followed by an interpretation in terms of culture and chronology, an attempt at a reconstruction of the socio-economic aspects of Britain in the 2nd millennium B.C., a discussion on excavation methods, and, in a final chapter on future policy, some very frank and forthright criticisms of certain aspects of our state-sponsored archaeology.

The book contains much perceptive comment, and a welcome synthesis and co-ordination of evidence widely scattered and previously unclassified. Copper and bronze tools and weapons such as knife-daggers are usefully discussed, and the stone battle-axes given a typology within a chronological framework (independently arrived at by the reviewer, who is also gratified to find his basic Beaker scheme of 1955–6 accepted here). There is naturally room for differences of opinion in the interpretations summarized in the chronological table in Fig. 56, but on the whole there is a welcome break away from the old chest-of-drawers arrangements, and the complicated overlapping of our Late Neolithic cultures with the earlier copper and bronze industries is emphasized. The illustrations are of a high quality throughout, and the author's drawings which make up most of the line blocks are attractive and informative.

Mr Ashbee pleads for a planned research programme, and a raising of official standards beyond that of employing excavators 'inexperienced and untrained in all but the barest rudiments of their craft', and of relegating the finds constituting the basic material evidence of our study to be 'incarcerated in the dust and decay of depository and deep-shelter' in our main national collection (it must be remembered that there is no National Museum of

British Antiquities). He is right, and if action ultimately depends on a responsible archaeologically educated public, he has written a book which will help such a body of informed opinion to come into being.

STUART PIGGOTT

ARCHAEOLOGY IN THE HOLY LAND: *By* KATHLEEN M. KENYON. *London: E. Benn Ltd.*, 1960, *pp.* 320, 56 *plates*, 66 *figures*. 35*s*.

This book, as might have been expected of an outstanding field archaeologist and distinguished, independent-minded scholar, is an impressive addition to the list of works on Palestine archaeology. Miss Kenyon's conclusions, always fresh and original, differ at times from the widely accepted views of Professor W. F. Albright, the greatest living authority on Palestinian and Biblical archaeology. Her latest book contains much that she has already presented elsewhere in scientific journals or books. But now, encompassed within the covers of a single volume, they provide a compact guide to her trend of thought and her pattern of argument, as her scholarship ranges from periods as remote as the Mesolithic to that of the final years of the Kingdom of Judah.

Dr Kenyon prefaces her book with the hope that in seeking to satisfy both the general reader and the professional student she does not fall between two stools. It is with satisfaction that this reviewer finds her seated firmly on one stool—that of the professional student. Her detailed and technical studies are of considerable importance for the student of archaeology, though they are studded with historical observations of lesser interest. These observations may appeal to the lay reader, but he can find them dealt with more comprehensively in other works, undisturbed by the kind of technical scholarship which Miss Kenyon offers as a mine of information to the professional.

The forty plates of line drawings of pottery, unequalled in this type of book, offer the reader a representative and comprehensive picture of Palestinian pottery from its beginning in the Neolithic Period to the end of Iron Age II. Incidentally, since Dr Kenyon employs her own terminology and dating of archaeological periods, it would have been helpful to provide a table comparing them with those in standard employment and those used for the neighbouring countries.

The introduction covers a wide range of subjects, including the Old Stone Age, archaeological research, methods for fixing the absolute chronology, and general principles of archaeological excavations. It is a pity that Miss Kenyon did not enlarge here on the methods of excavation— particularly her own—and did not give more details of ways of fixing absolute chronology with the aid of pottery and other purely archaeological methods. The two maps attached to this introduction are useful. But the one (fig. 1—Map of Near East, showing principal early settlements) leaves the area of Palestine almost unmarked except for Jericho; while the other (fig. 2—Map of Palestine, showing principal ancient sites) is incomplete and presents the whole of the sea shore from Ascalon northwards as a bare coastline; certainly sites like Jaffa, Tell el Qasileh, Tell Jerisheh, Tantura (ancient Dor) and Ez-Zib, should have been noted. The spelling of place-names on the map does not always tally with that in the text, and this may confuse some readers (Tell el Hesy—Tell Hesi; Tell Ajjul—Tell el Ajjul; Tell el Duweir—Tell Duweir, etc.), and incidentally, if Tell es Sultan is so spelt, Tell el Duweir, should be Tell ed Duweir.

The first chapter—'The Beginnings of Settled Life'—is enlightening, giving concise information on the now famous Pre-Pottery Neolithic Jericho, described in detail in Dr Kenyon's previous book *Digging up Jericho*. This chapter, however, also contains the results of discoveries during the 1958 season concerning the Mesolithic Period. These are of the utmost importance for the study of the origins and development of settled life in the Near

East, which, together with Perrot's discoveries in Mallaha (Eynan) and Stekelis's in Nahal Oren, will throw important light on the Mesolithic period.

The amazing fortifications of Pre-Pottery Neolithic Jericho remain an enigma, particularly the rock-cut ditch or fosse, 27 ft. wide and 9 ft. deep! Against whom was it designed, this defence device, engineered with such labour?

Miss Kenyon then deals with the next two periods, Pottery Neolithic and Chalcolithic. The reviewer would prefer the term Ceramolithic as more appropriate than Pottery–Neolithic. This would also obviate the need for the term Pre-Pottery Neolithic. The sequence would then be: Neolithic (the final pure stone implements), Ceramolithic (the first appearance of pottery), and Chalcolithic (the first appearance of copper), each period named after its characteristic archaeological material.

The 'Proto-Urban Period' is Dr Kenyon's suggestion for the next phase, part of which was until recently ascribed to Chalcolithic (the grey-burnished vessels of the Esdraelon ware), and part to the very beginning of the Early Bronze. Dr Kenyon argues here in favour of 'Proto-Urban', thus bringing it into line with Mesopotamian terminology (Proto-Literate) and Egyptian (Proto-Dynastic). But the suggestion is not a happy one. For Mesopotamia and Egypt, archaeologists employ historical rather than archaeological terminology (Old Empire, Early Dynasties, etc.). For Palestine the terminology used up to now is strictly technical, and the introduction of a terminology—for one phase only—based on cultural development seems inappropriate. There is much to be said for following Professor Wright, who would like to include the whole period in Early Bronze.

For the same reason this reviewer is not in favour of Canaanite and Israelite as terms for the Bronze and Iron Ages as suggested by other scholars. It is confusing when one has to define, for example, Philistine ware of the Israelite I period.

The chapter on the main phases of the Early Bronze Period summarizes in convenient form Miss Kenyon's published studies on Early Bronze Jericho, her critical re-examination of the EB strata of Megiddo, and her study of the copper weapons from Jericho and Tell el Hesy. Here is also a detailed treatment of the pottery of the period.

Incidentally, the appearance of a semi-circular bastion or external tower in the wall of Jericho, can hardly be called 'a very unexpected early occurrence of this type of addition to a town wall' (p. 107). The celebrated relief from Deshasheh already shows a Palestinian, or at least Asiatic, city, defended by a city wall with semi-circular towers or bastions. A similar bastion was found also in Ay. Nor is her statement that 'archery was certainly not in use then' consistent with the evidence in the same monument from Deshasheh.

The two chapters covering what is commonly called the Middle Bronze periods, are enlightening and detailed. Here the author deals at length—with the aid of drawings (figs. 37–42)—with her suggestion for the sub-division of the MB II (according to her terminology) into five phases of pottery groups—a subject discussed also in her Jericho I.

The chapter on 'The Late Bronze Age and the coming of the Israelites', introduces the reader to what might be termed the proper Biblical archaeological part of the book. The problem of Joshua's Jericho is dealt with rather briefly. This reviewer is of the opinion that one would look in vain for Late Bronze walls. The famous 'walls of Jericho' are almost certainly the former, Middle Bronze, massive revetment wall, which continued in use in the Late Bronze period settlement. The remnants of its houses and tombs were discovered by both Garstang and Kenyon. The rest of the archaeological data concerning the conquest is discussed with praiseworthy caution, although a more detailed analysis of the evidence would have been welcomed.

The chapter on the Philistines and the beginning of the Early Iron Age, utilizes already some of Mrs Dothan's observations on the Philistines, but it is regrettable that the excava-

tions at Tell el Qasileh, which shed important light on Philistine pottery and chronology, find no mention in Dr Kenyon's book.

Chapter ten begins with the startling statement that the period of 'The United Kingdom of Israel . . . was the only time in which the Jews were an important political power in Western Asia.' What of the periods of Ahab, of Uzziyahu and Alexander Janneous? This chapter deals with the most intricate and tantalizing (archaeologically speaking) period in the history of the kingdoms of Israel and Judah. Our dig at Megiddo confirmed Crowfoot's and Kenyon's opinions that the famous stables are not Solomonic but belong to a later period. On the other hand it showed that Solomon's city was of a considerable size and splendour and Kenyon's arguments about the rest of the Solomonic structures, were disproved. The description in this book of the Solomonic Megiddo should be corrected in the light of our report in the *Biblical Archaeologist*, XXIII (1960), p. 62 ff.

The final chapters, dealing with the divided Monarchies and their fall, touch on most of the important excavations. They would have been more complete had they mentioned the palatial building found in recent years at Ramat Rahel near Jerusalem—which is strikingly similar in plan to that of Samaria—as well as other, smaller but important sites.

Incidentally the Citadel of Hazor VIII had not 'continued in use down to the Hellenistic period, even when the rest of the site was abandoned'—p. 271; this citadel was destroyed in 732 by Tiglath Pileser III. Upon its ruins two more citadels were built, one (Strata III–II) in the Assyrian and Persian periods, and one (Stratum I) in the Hellenistic period.

The book ends with a useful appendix listing excavated sites. It is hoped that in a second edition, this list will include more of the many excavations carried out in Israel in the last ten years. Dr Kenyon's book does not replace Albright's *Archaeology of Palestine*, but it is certainly an important and scholarly companion to that book.

YIGAEL YADIN

NO STONE UNTURNED: An Almanac of North American Prehistory. *By* LOUIS A. BRENNAN. *Gollanz*, 1960, *pp.* xii + 370, 29 *figures*, 12 *plates*. 25*s*.

'. . . the wholly unjustified presumption that these arts and crafts could, by divine plan, be invented only once.' (Brennan, p. 64.)

'. . . we place no faith in the theory of duplicated independent invention.' (Gladwin, *Men Out of Asia*, p. 16.)

'(Dr Phuddy Duddy) . . . is a 100 per cent American in his unshakable belief that American Indians not only pulled themselves up by their own bootstraps, but also invented the boots and straps.' (Gladwin, *ibid.*, p. 21.)

Twenty years ago, few responsible scholars would admit that New World civilizations had developed anything but independently of those of the Old World. Shortly after that, the pendulum began to swing, and in 1947 Gladwin's entertaining book *Men out of Asia* preached the contrary view in no uncertain voice. A Trans-Pacific exhibition which was mounted at the American Museum of Natural History for the Americanist Congress of 1949 provided further propaganda in favour of diffusion, and, from then on, many have been at least receptive to the idea. We come full circle with Mr Brennan, who is convinced of the independence of American man, from most lowly beginnings over 30,000 years ago until 'as tenuous as a rainbow, something peregrine and exotic, something cryptically oriental' began to 'hover' over the high civilizations of Middle America and Peru. Brennan does not mention Gladwin and doubtless he would hate to be bracketed with him, but no one should read his book without also reading Gladwin's.

Let it be said at once, Brennan knows a great deal. He is an enthusiast for archaeology, he knows his literature, he writes volubly, and many of his comments are shrewd, but the book

is written to propagate a theory, and if anyone swallows it without some independent knowledge of the material, he will be deceived. Brennan makes a great use of radio-carbon dates, not always the latest ones (see, for example, p. 286. Many more Hopewell dates were available by 1958), but attempts to check them are greatly hindered by his tiresome policy of expressing them all as B.P. He is a journalist and he writes like one. In an effort to enliven his work, he spills a good deal of ink on curious speculations. 'It is a glittering temptation to do a fancy job and a job of fancy on the Adenans and the Hopewells', a temptation to which he succumbs, even introducing a touch of sex—'maidens who will be exchanged in friendly towns for other maidens that all the Hopewell land may be of one blood'—and not only here, since he populates Santa Rosa Island, now thirty miles off the coast of California, with the descendants of 'a forward adolescent who tampered with one of the females of his chief's harem' and was set adrift with her over 30,000 years ago!

Distractions of this sort apart, the whole thesis of the book depends on the validity of those occurrences which are claimed to represent the presence of man in the United States 30,000 years or more ago. They are the butchering of the dwarf mammoth of Santa Rosa Island, 'the very minimum number of the very minimum of types' of doubtful tools at Tule Springs, and Lewisville man who 'is a hammer stone, a flake scraper, a chopper, many hearths and the bones that were the garbage of his feasts'. All these are highly questionable as the work of man, and even if better evidence is discovered, as it well may be, it is still a large assumption to derive the Clovis and Folsom hunters and the gatherers of Danger Cave from such manifestations.

After these beginnings, everything is explained ingeniously by the interaction of factors developing in America, without outside additions apart from the cryptically oriental aura which I have mentioned. There are, however, various excursions into the outside world, which may well shake the reader's confidence in the author's judgment. He disclaims Atlantis and Mu, but cannot deny himself the thrill of like fantasies. On the strength of a core, which brought up what are said to be some freshwater diatoms from the ocean floor, it is suggested that there was a land bridge in mid-Atlantic across which the makers of Clovis points wandered to become the Solutreans! To account for the peregrine oriental rainbow, he imagines that recent work on the configuration of the Pacific floor warrants the suggestion that it came from Asia by way of a series of vanished islands. Direct voyages in junks would be so much easier!

This book falls between two stools. *Pace* the sub-title, it is not a text book of archaeology. If it were illustrated by caricatures instead of drawings of uneven quality, it might qualify as science fiction, like Gladwin's, but it is not nearly so amusing.

G. H. S. Bushnell

THE PREHISTORIC CHAMBER TOMBS OF FRANCE, a geographical, morphological and chronological survey. *By* Glyn Daniel. *London: Thames and Hudson*, 1960, pp. xix + 282, xxxii *plates*, 77 *drawings and maps*, *ref. and index*. 70*s*.

We welcome the publication of this book to which we have looked forward for some years. Our English and French colleagues alike will join in this welcome, and in his introduction the author rightly stresses the interdependence of megalithic studies in both France and the British Isles, and the impossibility of understanding the origin of British megalithic tombs without a good knowledge of their French counterparts. In spite of good regional efforts, there is still no detailed study of the subject as a whole written in France, although for several years such a work has been in preparation by Dr Arnal. If Dr Daniel is right to draw attention to the backwardness of French megalithic studies, he is surely overstating the case, in the present writer's view, when he says France is forty years behindhand and

thus justifying the necessity of what he himself calls foreign 'piracy' in French prehistoric archaeology. It is my duty to warn those young foreign field-workers whom our author is encouraging to write regional monographs on the megaliths of the Vendée, Maine, and Poitou, for example, that J. Chaigneau has embarked on such a survey in the Vendée, that Dr Gruet is working on the Maine tombs, and Claude Burnez in Poitou. In fact France is rapidly doing her best to catch up. But let us leave this good-natured argument here.

In the introduction to this book questions of morphological and chronological nomenclature are discussed: it is wise on the part of the author to refuse to concern himself with the fruitless argument that ranges round the attribution of megalithic tombs to the Neolithic, Chalcolithic or Bronze Ages. It is now becoming possible to use absolute dates, and this will become increasingly common as more Carbon-14 dates are forthcoming. It is naturally regrettable that the major part of this book had been written before the first Carbon-14 dates for megaliths were produced, and that, because of this, mention of the gap of several centuries which has appeared between the first Carbon-14 dates for megaliths and our previous chronological estimations, has had to be relegated to the last few lines of the preface.

After an interesting and copiously documented chapter upon the historical study of megalithic research in France, the description of the French megalithic tombs is approached geographically, by dividing the country into seven regions. It is easy to criticize in small details the manner of this division, as for example the placing of the Dordogne in the South-West region whereas, as the natural transition between the *causses* and the Centre-West, one might have expected to find it attached to one or other of these regions: but it is well to remember that no sub-division would have satisfied everyone. The first regional group dealt with is Northern France (taking in the Paris Basin), to the east of the Armorican massif and including the Channel Islands. The description of the Gallery Graves of the Paris Basin properly reaffirms the homogeneity of tomb-type and grave-goods: the question of the origin of these tombs cannot be considered as wholly solved—indeed the author is content to state the various theories and to indicate his own preference for South Iberian prototypes and a penetration into the Paris Basin by way of the Lower Seine. There can, however, be no possible doubt about the maritime origin of the small group of Passage Graves in west Normandy, in the same way as there is no doubt of their complete independence from the Gallery Graves of the rest of Normandy. The richness and variety of megalithic architecture in the Channel Islands is rightly stressed.

The same wealth of types occurs in Brittany, the subject of the next chapter; here the nomenclature of tomb-types adopted by Dr Daniel is largely based on those of Le Rouzic and Forde, by simplifying the former and expanding the latter. He reaffirms the affinities of the Transepted Gallery Graves (which he here re-names the *Keriaval-Herbignac type*) with the Gallery Grave family, in spite of the fact that most French scholars working in this field would prefer to classify them with the five varieties of tombs now grouped in the author's Group 2—*Passage Graves of non-classic type*. The southern and maritime origin of the Breton Passage Grave is universally accepted; the genesis of the Breton Gallery Graves poses more difficult problems; our author puts forward the suggestion that they reached the Armorican peninsula in three successive waves, the first a coastwise movement from the Gironde, the second starting from the same place but going by land via the Centre-West and the Loire valley, while the third is part of the same sea-borne movement which he thinks was responsible for the Paris Basin tombs. It seems to us that when we read (p. 110) the statement that Gallery Graves in Brittany are perhaps as old as the oldest Passage Graves, the author is needlessly discounting the evidence of the differential geographical distribution of these two types, to which he has hitherto attached great impor-

tance. The long chronology suggested by the Carbon-14 dating for the Passage Graves obviously allows an easier comparison with the dates known for the Severn-Cotswold tombs, which are considered to be, at least in part, of Breton origin.

The Loire valley, briefly described, emerges as a province of Gallery Graves, and this is certainly so if one considers the *dolmens à portique* of Gruet's terminology as a variant of this type; but the evidence for this is not wholly convincing. The chapter devoted to the Centre-West is largely descriptive, and it would seem the author has not found it possible to set out so clear a synthesis of these tombs as he has in the preceding chapters. The work of Claude Burnez will help us here. The South-Western group is certainly very varied, especially if we take in the Dordogne, and it is perhaps an over-simplification to regard all these tombs as derived from the Gallery Graves of the Aude.

Among the megalithic tombs of the Midi, Dr Daniel distinguishes five geographical groups, first the Gallery Graves of the Aude, and secondly the Arles-Fontvieille tombs, both of which were used for a long time and whose origins were certainly pre-Beaker (but they should surely not be dated too early—we know of no Chassey grave-goods from the first group, and the presence of Chassey material from the second is not entirely proved), thirdly the Hérault-Gard group (and here he follows closely Arnal's descriptions), fourthly a group in eastern Provence, and lastly the group of Eastern Pyrenean tombs of uncertain affinities.

The south of the *massif central* has one of the densest concentrations of megalithic tombs in the world, and Dr Daniel underlines the fact that the departments of the Lot and the Aveyron have each more megalithic tombs than the whole of England and Wales put together. The types here are, however, not very varied; the commonest type is a rectangular chamber of a smallish size—the origins of these tombs is to be sought at once in the Passage Graves of the Hérault and the Gallery Graves of the Aude—from which many of these tombs derive their long barrows.

The regional description is followed by a chapter of conclusions, from which the history of the megalithic colonization of France appears much more complex than as it was set out by the author twenty years ago. In refusing to define an absolute chronological date for each type, it would appear to me that Daniel reacts too vigorously against the work of Arnal, who has, however, gone too far, without any doubt, in the opposite direction. Daniel finally proposes a chronological scheme divided into five phases and covering from 2300 B.C. to 750 B.C.; this does not satisfy me entirely and it looks as though the absolute chronology would have to be considerably lengthened in the near future.

The criticisms of detail made here, many of which are largely the personal opinions of the reviewer, in no way detract from the value and interest of this book, which certainly fills a long-felt need. The very full bibliography of forty pages is especially valuable, followed as it is by a list of sites with their geographical location. The presentation of the book is first class, the line drawings and plates are both abundant and of the highest quality, and here one cannot deplore spelling mistakes in place names, in happy contrast to certain other works by foreign archaeologists on French prehistory. One would, however, have liked more uniformity of scale in the reproduction of tomb-plans.

GERARD BAILLOUD

EAST ANGLIA. *By* R. RAINBIRD CLARKE. (Volume 14 in the 'Ancient Peoples and Places' series). *London: Thames and Hudson*, 1960. *pp.* 240, 60 *photographs*, 43 *line illustrations*. 30*s*.

As a study in the archaeology of a particular region of early Britain, this volume by Mr Rainbird Clarke, Curator of Norwich Museums, is a worthy addition to this well-accepted

Series. By his family background—his father, W. G. Clarke, was author of the classic *In Breckland Wilds* and founder of the East Anglian Society that developed into the present Prehistoric Society—and his training at Cambridge, as well as by his official position and his unrivalled experience as Secretary of the Norfolk Research Committee, Mr Clarke is admirably qualified for undertaking this book. He has written it with a modesty all the more attractive for his obvious mastery of his material. For the greater part of its perimeter the region dealt with in this volume is defined by natural features—on the north and east by the North Sea and on the south by the Stour and its estuary; only on the west, in the southern part of which it overlaps with Fox's Cambridge Region, is it artificially defined by the county boundaries of Norfolk and Suffolk. In presenting his region Mr Clarke very properly emphasizes how its physical character has influenced its early history: how, for instance, through its open coast and welcoming estuaries, not least among them the Wash, it positively invites invaders, from a wide arc of the Continent ranging from Northern France to Denmark; and how, again, its lack of anything comparable with the chalk uplands of Wessex or Sussex has affected the archaeological record. Yet, equally, he stresses how many of its physical characteristics have been shaped or at least modified by man: how its comparative treelessness, only recently and locally remedied by the Forestry Commission, is the product of deforestation initiated by Neolithic man; how the incursion of the sea has been restricted by sea-banks; how rivers have been straightened and artificial waterways cut; and how the Norfolk Broads themselves were formed by the drowning of extensive medieval (9th–14th centuries) peat-cuttings.

The wealth of E. Anglia in flint and its abounding Pleistocene deposits have caused it, from the time of Frere, to take a leading place, alongside the Thames Valley, in the study of the Lower Palaeolithic sequence in Britain, even if as Mr Clarke points out in connection with the Sub-crag and Cromerian flints it also has been the occasion of misguided zeal. Mr Clarke is also rightly critical of a number of finds once claimed as Upper Palaeolithic, and the fact must be admitted that adequately documented finds for the Late Glacial period have yet to be made in this region; nor, despite a few notable finds and numerous stray flints, is our knowledge of the Mesolithic cultures of this area more than rudimentary.

Mr Clarke's series of distribution maps is particularly valuable for illustrating the expansion of the later Prehistoric settlement. They show, amongst other things, how Neolithic occupation was concentrated on one hand in the west from the fen-edge and Breckland to the Norfolk edge and on the other in the extreme south-east, a territory now partly submerged by the sea; how closely traces of the Wessex Culture relate to the course of the Icknield Way; how the Bronze Age people spread over much of the land north of the Waveney; and how it was not until Roman times that roads were driven through the clay lands of High Suffolk, settlement even then being confined mainly to the river valleys.

As a narrative the book really comes to life during the Iron Age, when historical movements of people can first be inferred with reasonable certainty. One may particularly commend the way in which Mr Clarke handles the difficult transition from the Prehistoric to the fully Historic period, an object lesson in the judicious use of archaeological evidence in reconstructing phases of the past variously documented by the written word; in a few thousand words of plain narrative he takes us from the Pre-Roman Iron Age to the reign of King John without our being aware in any obtrusive manner of the changing nature of this evidence.

The numerous well-reproduced half-tone illustrations remind us that during the last twenty or thirty years East Anglia has yielded several of the greatest treasures of British Archaeology, notably those from Ringstead, Snettisham, Mildenhall and Sutton Hoo. The Bibliography listing articles on the periodical literature makes the reader free of the

main published sources. Mr Clarke has clearly shown how valuable a study of regional archaeology within our small but astoundingly varied land can be and one may hope for many more such books. A useful addition would have been a folding map showing the location of the main visible sites in relation to modern roads: one of the most obvious ways in which the layman can respond is after all to drive out and see for himself.

GRAHAME CLARK

THE ARCHAEOLOGY OF WEAPONS: Arms and Armour from Prehistory to the Age of Chivalry. *By* R. EWART OAKESHOTT. *pp.* 360, 23 *plates*, 179 *figures. London; Lutterworth Press*, 1960. 45*s.* net.

A good many writers on archaeology have had perforce to say, and occasionally to study, something about arms and armour. It is rare, however, to find an author who couples a really exhaustive knowledge of arms with sufficient archaeological understanding to produce a book of the quality of this one. Mr Oakeshott is at once a collector and a lover of early swords; he takes pleasure in the fine examples he has been able to admire in other collections or acquire for his own, and he succeeds admirably in conveying much of that pleasure to the general reader. There is no risk here, as sometimes there is with such a subject, that the book may develop into a mere *catalogue raisonné* of outstanding pieces in public or private ownership; the archaeological approach leads author and reader alike to consider the development of hand-to-hand fighting, and the extent to which existing weapons can help us to a clearer knowledge of those who made and used them.

Mr Oakeshott's preoccupation is mainly with swords. Other arms are incidentally touched upon, but with no pretence of the detailed consideration that is given to the real weapon of the author's fancy. A swordsman himself, he makes clear from the outset the varying ways in which a sword can be used, and differentiates convincingly between the cutting and thrusting types, from Minoan times onwards. He reminds us that for many centuries the sword was used for attacking *only*, its wielder relying for the protection of his person, either on armour or some independent defence controlled by the left hand, so that it is a mistake to expect from an early sword the particular type of mobility required of a weapon used for deflecting thrusts as well as delivering them. The treatment of Celtic and Dark-Age swords generally is more detailed, and more helpful than that of the Greek and Roman examples. It may be questioned whether the kukri-like blade of the Greek *kopis* or *machaera* was in fact introduced to the East by the armies of Alexander, as we are told on page 50, since the well known ' Darius ' vase at Naples illustrates just such a weapon in the hand of the Persian king's personal bodyguard.

Gaulish and Romano-Celtic examples are cited from unfamiliar sources, and when we get to the period of the Sagas, Mr Oakeshott is able to combine practical, historical and literary knowledge. Particularly fascinating is his elucidation of a passage in the Saga of Hrolf Kraki by explaining what happens if one breathes on the blade of a pattern-welded sword, and the Saga stories of the continued use of ancestral weapons, and the practice of attaching armulets or ' life-stones ', are illustrated by the corroborative evidence of excavation. It is perhaps ungrateful to complain of lack of illustration when Mr Oakeshott's drawings are at once so numerous and so good, but it would have been pleasant indeed to have seen, for comparison with the other types depicted, a picture of one of those disc-pommels normally attributed to the 12th century and later, but now antedated by being found in unquestionably 10th-century graves.

As the Middle Ages proceed, the typology becomes rather formidable. Wheeler's original seven types of Viking sword have been increased to nine on page 133, the period from 1100 to 1325 gives us five more, and by the end of the Middle Ages the number has

been brought up to nineteen. In addition to this there are twenty-two types of pommel, some of these subdivided in their turn, in a manner reminiscent of that distinguished archaeologist to whom we are reputedly indebted for the subdivision of Iron Age B Flat Minor. It is rather bewildering at first glance, and impossible for any but the expert, to carry in the head, but the classification is of undoubted importance, enhancing the book's value as a useful work of reference. It is a book to be read and re-read, not only for its interest and style but because familiarity with its construction is the only way to find what one wants in it without delay and frustration, the Index being well below the standard of the rest of the work.

The author's classical scholarship is somewhat rusty, if he really thinks the word ' barbarian '—a direct adaptation from the Greek—had anything to do with beards, but his most surprising statement is the one ascribed to an unnamed ' chronicler ', that William I himself struck Harold on the leg at Hastings. We must have better evidence than this, if we are to discount the statement of William of Malmesbury, that the Conqueror denounced the stroke as cowardly and shameful, and dismissed the striker from his service in disgrace.

MARTIN HOLMES

ASSYRIAN PALACE RELIEFS and their Influence on the Sculptures of Babylonia and Persia. *Text by* R. D. BARNETT, *photographs by* WERNER FORMAN. *London: Batchworth Press Ltd.*, 1960. *pp.* 36, 173 *plates*, 23 *colour plates*. 42*s*.

In many ways this is a very attractive book. It contains a selection of photographs of the British Museum Assyrian reliefs from Nimrud, Nineveh and Khorsabad, and the bronze gates from Balawat, with an excellent introductory note by Dr Barnett. The somewhat misleading subtitle was obviously appended to allow for the inclusion of 22 colour plates of glazed tile reliefs from Babylon and Susa and one of a great bull capital from the latter site. None of the Persepolis reliefs is included, and the text as well as the illustrations is mainly concerned with Assyrian sculpture. The colour reproduction is unfortunately of poor quality.

The standard of black and white photography is very uneven. In general, the close-up photographs of small details are extraordinarily good and the larger scenes poor. Bad lighting has in many instances resulted in a fuzzy picture with detail obscured. It is a pity that the plates were not selected by Dr Barnett, who is well acquainted with what has previously been published and with the earlier standard of photography. The 9th- and 8th-century reliefs, for example, are far better reproduced in the original British Museum publications; for the most part this is true also of the Balawat gates, although the excellent details in the present volume are a valuable supplement to the plates of the original publication. It is useful to have, in the text, Dr Barnett's new arrangement of the bands of the gate. The 7th-century reliefs have, of course, not as yet appeared in the British Museum series, and some have never been published. For this reason it is particularly praiseworthy that the detailed photographs of the Aššur-bani-pal sculptures are the best in the book.

It is, however, one of the defects of this type of book, in which the illustrations are chosen more or less at random by the photographer, that although an attractive volume of pictures may be produced, the resulting arrangement makes it difficult for the more serious student to visualize the reliefs as a whole or in the groups in which they were meant to be seen. This difficulty is particularly evident in the 7th-century narrative sculptures, and it has not been mitigated by the insertion of irrelevant colour plates in the middle of continuous friezes and by the actual separation of photographs of contiguous objects. A particularly annoying feature is the siting of the plate number on the inside margin, so that the rather large volume has to be opened in full to search out a single illustration.

Dr Barnett's useful and informative introduction briefly discusses the history of palace decoration and the use of stone reliefs, the excavation of the Assyrian palaces, particularly in the 19th century, and the sculptures themselves. A short section at the end mentions Neo-Babylonian and Achaemenian architectural decoration. It is an excellent commentary and only a few points need to be mentioned.

Fresco decoration did not as suggested by Dr Barnett (p. 9) retreat entirely into a subsidiary place in the larger Assyrian palaces. This is demonstrated particularly by the magnificent throne room frescoes at Khorsabad, and it is clear also from the excavations in the North-West Palace at Nimrud that the throne room sculptures had once been surmounted by frescoes. The excavations of the British School of Archaeology in Iraq have extended the plan of the North-West Palace far beyond that dug by Layard and referred to by Dr Barnett; reference to the articles and plans in *Iraq* and the summary and plan in Professor Mallowan's *Twenty-five Years of Mesopotamian Discovery* would have made the discussion clearer and increased the usefulness of this section. It is perhaps a printer's error that in Dr Barnett's description of the North-West Palace, the terms for the courts have become reversed (p. 10). The *bitanu* is the inner court discovered by Layard and the *babanu* the huge outer court between the throne room façade and the administrative wing excavated by Mallowan and referred to as ZT. A further point is that the throne room façade consists not only of Phoenicians with monkeys (the only relief from this group in the British Museum) but of a number of foreigners carrying various tribute; some, Syrians, bear heavy ingots, bars, rings, and cruciform ornaments of precious metal on trays. The two doorways to the throne room were, of course, flanked by great *lamassu*. The whole façade has now been restored at Nimrud by the Iraq Antiquities Department and these sculptures, left on the site by Layard and re-excavated by Professor Mallowan, are now in their original positions, along with a cast of the British Museum slab. The south wall of the throne room consisted of more than the double band of scenes mentioned by Dr Barnett (p. 10). The central motif is a repetition of that behind the throne, a large group portraying the king standing on either side of the sacred tree between winged spirits. These sculptures are also at Nimrud and have been restored *in situ*. It is not clear what the author means by his statement that the double band on this south wall ran 'at about eye level'. The original slabs comprised a small relief at top and bottom, separated by the so-called Standard Inscription of Aššur-nasir-pal; these stood along the floor to a height of about 7½ ft., as did all the others. One minor point should be corrected in the account of the recently discovered Balawat gate (p. 14). The gate was not completely jammed open; one half was closed and the other only partially open.

There are a number of errors in the catalogue. Some of the attributions are wrong; for example, a number of reliefs said to come from the North-West Palace throne room at Nimrud came from elsewhere in the building: pl. 1 from Room T, and the large group of king, cup-bearer, and keeper of the bow (pls. 28–31), with a number of others like it, from Room G. Pl. 48 is a detail of pl. 46 and not 47. Pl. 8 is not a detail of pl. 7 as stated, but a much larger relief of a similar figure; nor is pl. 5 a detail of pl. 3.

In spite of these criticisms the volume remains a very welcome addition to the all too meagre number of books on the Assyrian sculptures. For the general reader it provides, for the first time, a comprehensive introduction to this remarkable branch of ancient Near Eastern art, and for the specialist the really excellent detailed photographs of the Sennacherib and Aššur-bani-pal reliefs, side by side with the earlier material, make the book well worth the quite reasonable price.

JOAN OATES

ROMAN COINS FROM THE EARLIEST TIMES TO THE FALL OF THE WESTERN EMPIRE. Second edition. *By* HAROLD MATTINGLY. *London*, 1960. *pp.* 303 + 64 *collotype plates.* 63*s.*

Thirty-three years have passed since the first edition of this work was published, with its sombre prefatory reminder that Mommsen, in writing his great study of the Roman coinage, had done for numismatists what they would not do for themselves. The 1927 edition was, as Mattingly stated, a great adventure from which he hoped that new research might follow. No one can say that numismatists have neglected the Roman coinage in this last generation. Just how much new work has been done since then, stimulated by Mattingly's own constant output of new work and new ideas, can be seen vividly in the 1960 edition. The first chapters of each of the three main sections have been rewritten; the rest of the text has received numerous smaller changes; and the bibliography, concentrating on more recent works, has been totally revised. And all this has been done with an astonishing expertness. The sweep is wide, the experience deep; argument is acute and suggestive; and the occasional insistence on a personal view is well tempered by lightness and humanity of touch.

It was of course to be expected that the whole chronology and assessment of Republican coinage would be fundamentally altered, since it was Mattingly himself, with Robinson, who in 1932 effectively transferred the first denarii from 269 B.C. to 187. The first denarii are now (1960) put even later, *c.* 169, on the evidence, mainly, of overstruck coins and of Plautus. One may wonder whether the evidence for this even later date is as secure as it is said to be: the date of the Plautine reference is uncertain, and Livy, of course, did not record monetary equivalences 'in 197' (p. 5). With the revised denarial date comes a mass revision of the whole subsequent denarial coinage, now given, partly on stylistic and partly on historical grounds, to a wide variety of mints, many of them as admittedly hypothetical as those to which the post-269 didrachms are tentatively given (pp. 43 ff.). Mattingly himself is aware of difficulties: triumviral moneyers strike at more than one mint, on this construction, and he would still say that unity of control must not argue, even in such early times, against multiplicity of mints (p. 31). Here the numismatist still has much to do. The study of dies, so fruitful elsewhere, has hardly touched the Republic, and until mutually exclusive groups have been defined by die-study a primary section of evidence for multiple mints will not have been produced.

Revision of the early imperial section has adroitly taken account of all the major work produced in recent years, though Grant's most important contributions—the view of official bronze coinage as being dependent on imperial stimulation, and the very wide circulation of the principal official bronze issues—perhaps rate a rather stronger credit-line. Mattingly is still, it seems, on secure ground in his treatment of Augustus's western gold and silver in general (together with the question of finds of dies) and of the mint of Lugdunum in particular, and he has pondered afresh the question (previously asked by so few) of what constitutes a mint. On such matters as the occasions of issue, the circulation, and the information-content of early imperial coins he comments wisely: for example, he reminds the historian (who still betrays great and often inexplicable difficulty in distinguishing between the different standpoints of the wholly pro-imperial coinage and the normally anti-imperial historical writers of Rome) of the importance of noticing what the coins comment on and what they omit.

For the late empire, revision centres around the monetary systems of the 3rd and 4th centuries. Here our interest has recently been sharpened by Bolin's *State and Currency in the Roman Empire*, with its thesis that gold and silver were always substantially overvalued, and that the relative values of copper coins depended on whether they contained

any silver or not. Bolin's book probably antedated Mattingly by a period of time too short to allow its full impact to be felt. As it is, Mattingly 1960 is perhaps less persuasive than Mattingly 1927, especially in connection with the XX.I (or XXI) pieces known as folles: it simply does not seem possible that this formula can = 1 sestertius of 2 denarii (p. 128, cf. p. 217), and indeed there is little doubt that XX in Latin must = 20, though 20 of what may not (even after Bolin) be certain. And, if XX.I does not mean a piece of 2 denarii, Mattingly's monetary system for the period will need reconsideration.

The few points here mentioned will serve to show that this is not a second edition that has been merely tinkered with. In many respects it is a new book, coming from a scholar who has never been afraid of taking a fresh look at old problems; and the integration of the new material with the old is very skilful. The production, moreover, is good, apart from a very few misprints (most obviously in the bibliography), and the erroneous retention of the 1927 denarial dates on the relevant Republican plates; the plates themselves, while not so clear as the 1927 originals, are adequate and give a remarkably wide view of the Roman coinage in all its aspects. This second edition is good for a life as long, authoritative and helpful as the first.

C. H. V. Sutherland

THE ANTONINE WALL: A HANDBOOK TO THE ROMAN WALL BETWEEN FORTH AND CLYDE AND A GUIDE TO ITS SURVIVING REMAINS. *By* Anne S. Robertson. *Glasgow Archaeological Society*, 1960. *pp.* 92, 4 *plates*, 22 *figures*, 1 *folder map*. 6*s*.

Time has dealt harshly with the Antonine Wall. Doomed as an earthwork to gradual attrition by cultivation and natural agencies, it has been mercilessly hacked to pieces by industrial development during the last two centuries; so much so that at the present day only intermittent stretches of the Rampart and Ditch, and only one of the nineteen associated forts, are sufficiently well-preserved to impress the casual visitor. Now, however, a wind of change is blowing across the isthmus. Official bulwarks are being hastily erected against further encroachment, and a planned programme of excavation, covering both rescue and research, has been in operation for several years. The time is therefore ripe for a guide-book of this kind, modest in price and slipping easily into the pocket, which is intended not only to serve the immediate needs of tourists but also to inform students of Roman Britain of the advances made in the knowledge of the Wall since the publication of Sir George Macdonald's classic survey in 1934.

The book opens with a brief account of the nature and history of the Wall, and then describes the surviving remains from east to west—the order in which they were built. The constituent elements are well described, and we are very properly reminded that the three interval fortlets so far discovered on the line of the Wall all cover unusually long distances between pairs of forts, so that it is premature to regard them as members of a continuous series. A brief explanation of the purpose of the stone foundation would, however, have been useful, particularly in view of the lack of any similar provision on the Turf Wall of Hadrian, and a reconstruction drawing would have been preferable to the photograph of the model of Balmuildy fort. The profound changes which have taken place since the 1930s in our knowledge of the organization and history of the Wall are reflected in the historical section, where controversial issues are wisely avoided. Nevertheless the pace of current research is such that Miss Robertson's narrative has inevitably been overtaken by new discoveries at one or two points. Thus Dean House has now been eliminated as a possible site for the missing Kinneil fort, and excavations at Mumrills have provided grounds for thinking that

the first destruction of the Wall was not the work of the North Britons but of the Roman garrisons themselves, prior to a temporary and orderly withdrawal from Scotland. And it is becoming increasingly difficult to sustain the idea that the chain of *praesidia* built by Agricola some sixty years previously determined the siting of the Antonine Wall forts. Indeed, the identification of structural remains of these *praesidia* is one of the most pressing problems confronting mural archaeologists in Scotland.

The description of the actual remains is admirably done, and the text is amply illustrated by maps and by plans of the excavated forts. In this day and age, however, there is little advantage to be gained from traversing the entire length of the Wall on foot, and few persons are likely to make the attempt. A brief appendix listing the best preserved portions of the Wall, and instructing the motorist on how to get there, would not have entailed much extra work, and its inclusion might perhaps be considered when this valuable little book is reprinted.

KENNETH STEER

CRETE AND MYCENAE. *By* S. MARINATOS, *translated from the Greek by* J. BOARDMAN, *photographs by* M. HIRMER. *Thames and Hudson*, 1960. *pp.* 178 + 10 (*Index*), 421 *illustrations including* 52 *coloured.* £6 6*s.*

A most nostalgic book for such as know Crete and the Argolis, and surely a spur and incitement to those who have not yet visited those magical haunts. The frontispiece by Professor Hirmer reproduces the wonderful view of Mount Ida and the Kamares Cave as seen from Phaistos. Specialists will be more interested in the splendid coloured plates of the Kamares vases from Doro Levi's recent excavations, the profile and full view of the plaster head from Mycenae, the lily fresco from Amnisos and the Mycenae daggers, or among the monochrome photographs the enlarged gems, the Gazi figurines and the wine-press of Vathypetro. Professor Marinatos gives a well-balanced account of the development, floruit and decline of Minoan culture, only a summary of course, but his exhaustive knowledge of the subject permits him to insert some pregnant suggestions such as that the intervals of 7 to 10 miles between sites may have corresponded to the divisions of the Minoan administrative districts, the popularity of the mariner's profession attested by the numerous representations of ships on seal-stones, the analogies between traditional Cretan names such as Dictynna and Akakallis on the one hand, and Linear Script A names such as Pipituna and Asasara on the other, the identification of the pair of goddesses with the 'Cretan mothers' referred to by Diodorus, the suggestion that the pent-house roof on some of the houses of the Town Mosaic probably represented a summer bedroom, that the Greek word for iron may reflect the meteoric origin of the metal first used, that the doorway to a Homeric megaron was hinged from above, and that Elgin's bull slabs in the British Museum may have originally been placed above the doorway of the Treasury of Atreus. Many of these suggestions, of course, are very debatable, especially the last one but in favour of it may be remarked the scarcity of the spiral bands among the existing architectural fragments. Wace is wrong in saying there is no room for the slabs, but there is no positive evidence they were placed in that position.

The author develops at some length his theory, which I believe to be reasonable, that the devastation visible on so many Late Minoan sites was caused by the great eruption of Thera, much greater than that of Krakatoa to judge by the size of the crater and the subsequent earthquakes and tidal waves. This disaster he dates about 1520 B.C. and he may be right, though I think arguments could be advanced for a somewhat later date.

The final section of the book consists of an excellent *catalogue raisonné* of the objects

illustrated together with essential references and some plans which are not usually found in the more general text books, such as the plan and section of the Eileithyia Cave, and plans of the Tylissos villas and the Acropolis at Arne. Specialists will welcome the new material from Phaistos and Pylos though perhaps more might have been said on the recent discoveries near the mouth of the Kairatos. The general public, even those not particularly interested in archaeology, will revel in the fine printing and splendid plates, both old favourites such as the Mycenae daggers and gold work and lesser-known masterpieces such as the sword from Skopelos, the fantastically beautiful example of Matz' 'torsion' decoration on pl. 20, and the shrine in the Giamalakis collection. Misprints such as Stylios for Stylianos on page 8 and *ὀρσοθρύη* for *ὀρσοθύρη* on page 111, are few and unimportant.

On page 12 there are some curious understatements of the height of the Cretan mountains; Leuka is nearly 8000, Dicte nearly 7000 ft. and Ida (Psiloriti) not only nearly 7000 ft. but over 8000 ft. (the height given on page 115, namely 8179 ft. is presumably correct).

The reference to Cretan fauna seems inadequate. Hedgehogs, rats and mice are perhaps intentionally omitted as vermin, but the Cretan shrew the only animal peculiar to the island deserves a mention, and since the weasel is mentioned the marten should also be recorded.

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